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THE PRESENT POSITION IN PROBABILITY THEORY *

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MOST books on scientific method contain at least a chapter on probability theory. Most books on modern physics assume a knowledge of probability theory and claim to derive probability distributions. A casual observer might be led into supposing that the authors would be reasonably up to date in the subject. In fact, however, they seldom show any knowledge of it that is less than fifty years old, and some make it quite clear that the work of the last two hundred years has not yet attracted their attention. It is still widely believed that probability is an easy subject, and this is far from being the case.

I think that the greatest forward step in the general theory since Bayes's paper of 1769 was McColl's introduction of a notation that makes the data explicit. This was modified by W. E. Johnson. I use the form $P(p|q)$ to denote a number describing the reasonable degree of belief in p on data q. I must point out some misinterpretations at once. The idea of a reasonable degree of belief intermediate between proof and disproof is fundamental. It is an extension of ordinary logic, which deals only with the extreme cases. But even now people argue that different people have different degrees of belief and that therefore the whole subject is illusory. My reply is that if arithmetic was treated with as much carelessness as probability theory people would differ as much about the multiplication table. Ordinary degrees of belief are mostly guesses, even when the data are stated ; that is why the word ' reasonable ' is put in. But within scientific practice a considerable amount of agreement is found ; and at a more elementary stage a person in ordinary life manages to convey information to others without serious misunderstanding or

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disagreement in more than a few cases a day. (Desires, of course, are quite a different matter.) There is a solid mass of belief reached inductively, ranging from common experience and the meanings of words, to some of the most advanced laws of physics, on which there is general agreement among people that have studied the data. But these people themselves cannot say just how they have arrived at their conclusions. If they attempt any explanation it is one that could not possibly work. The problem of probability theory is to find out whether a formal theory is possible. Such a theory would be impersonal in the same sense as in ordinary logic : that different people starting from the same data would get the same answers if they followed the rules. It is still often said that no such theory is possible. I believe that it is possible, and that inattention to the progress that has actually been made is due to an active preference for muddle. But I should comment on some special types of objection. A formal theory must start with at least one primitive notion and at least one axiom. It is then argued that the theory has no basis unless the concepts are all defined and the axioms proved. This argument would obviously lead to an infinite regress if it was applied to any theory whatever. On the other hand, the condition leaves the matter vague until we have decided just how to begin. If we assert a probability for a law, presumably a high one, on some experimental evidence, somebody can ask ' what was the probability before you had that evidence ? ' He can then drive us back to a position where we have to assess a probability on no evidence at all. When we once understand what we are trying to do, this is seen to be a legitimate question. A numerical statement of a probability is simply an expression of a given degree of confidence, and there is no reason whatever why we should not be able to say within our language that we have no observational information. The idea that there is any inconsistency in assigning probabilities on no evidence is based on a confusion between a statement of logical relations and one on the constitution of the world.

Another objection is that the initial probabilities are arbitrary and introduce an arbitrariness into the whole theory. With this may be taken the objection that the theory is inconsistent. One objection says that more than one set of assessments is possible ; the other says that no set is possible. Curiously, people can be found to make both these objections simultaneously. The answer now appears to be known. With any set of initial probabilities the consistency of the axioms of probability theory can be proved (subject to the consistency of pure mathematics, which we take for granted). The only additional rule is that we must not try to assess probabilities on self-contradictory data, and we may willingly forgo that pleasure. It would be possible to choose the initial probabilities in an infinite number of ways, each separately consistent ; the problem is to try to choose the best.

I must say again that there is no question of proving the postulates. The position is the same as in the formalisation of pure mathematics. In this we must have a starting point, and Whitehead and Russell laid down the principle that when there is a choice of postulates we should choose those that enable most consequences to be drawn. The choice in their problem was such that it should lead to proofs of the essential propositions of pure mathematics. In the problem of induction the principle would say that the postulates should be taken in as general a form as possible subject to their leading to the principal types of scientific inference. Any set satisfying this condition I should describe as good. Subject to this criterion, I should describe one set as better than another if it requires fewer special rules. It may still turn out that there are many equally good methods in this sense. Curiously, if this happens there need be no great difficulty. Once the alternatives are stated clearly a decision can be made by international agreement, just as it has been in the choice of units of measurement and many other standards of reference. The object is not to prove that scientific method is right, but to tidy it up.

The problem of the choice of the best assessment of initial probabilities is not yet solved. But we have several important clues. The first goes back to two fundamental papers of Broad[1] on Laplacian sampling. If we are sampling a class of n members, r of which have the property ϕ, and a sample of m contains l with the property, and all values of r from 0 to n are taken to be initially equally probable, it can be shown that the probability, given the sample, that the next member will be a ϕ is $\frac{l+1}{m+2}$; and if $l = m$, the probability that all members are ϕ $(r = n)$ is $\frac{m+1}{n+1}$. When $l = m$ and is large $\frac{m+1}{m+2}$ is nearly 1, and this result had been widely regarded as a justification of induction. Broad pointed out that the general law under consideration is $r = n$, and that this would never acquire a high probability until m approaches n—a general law would never acquire a high probability until nearly the whole class has been examined.

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[1] C. D. Broad, *Mind*, 1918, **27**, 389-404 ; 1920, **29**, 11-45

The answer is obvious. The uniform assessment of initial probability says that before we have any observations there are odds of $n - 1$ to 2 against any general law holding. This expresses a violent prejudice against a general law in a large class. If we modify the rule to a statement that the general law has an initial probability $k > 0$, independent of n, we find that the probability that $r \neq n$, given $l = m$, is of order $1/m$, so that a general law can acquire a high probability on a moderate amount of evidence.

Wrinch and I[1] showed in 1921 that a similar difficulty arises even more acutely for quantitative laws, where the observable quantity is capable of a continuous range of values, and can be dealt with in the same way. The main theorems are that if q is a law, $p_1, p_2 \ldots$ successive verifications of results predicted by the law, then if $P(q|H) > 0$,

$$P(p_n \,|\, p_1\, p_2 \cdot \cdot \cdot p_{n-1}\, H) \rightarrow 1\,;$$

and that if q predicts p, that an observable will be in a range of length ϵ, $\sim q$ that it will lie in a much greater range E, then

$$\frac{P(q\,|\,pH)}{P(\sim q\,|\,pH)} = O\left(\frac{E}{\epsilon}\right)\frac{P(q\,|\,H)}{P(\sim q\,|\,pH)}.$$

Thus if $P(q|H)$ is a moderate fraction and E/ϵ large, a single verification will make $P(q|pH)$ nearly 1. (Not exactly 1; ϵ is never zero because no law predicts exact values for actual observations.)

Thus the adoption of a positive initial probability for a general law accounts at once for two of the most important features of induction. It explains both how a crucial test may send the probability of a law nearly up to 1, and how successive verification of predictions, even if not separately crucial, may lead to probabilities near 1 for further predictions. These successes strongly indicate that we were already on the right lines. But the fundamental principle leads to a curious consequence. Probability 1 denotes certainty, and that of the disjunction of any finite set of exclusive laws cannot exceed 1. Hence, by the addition rule, the sum of the initial probabilities of any finite set of laws, however many, is $\leqslant 1$. This implies that the possible laws form a finite or enumerable set; in the latter case their initial probabilities are the terms of a convergent series with sum $\leqslant 1$.

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[1] D. Wrinch and H. Jeffreys, *Phil. Mag.*, 1921, **42**, 369-390

There can be no empirical objection to this, because the laws that can ever have been considered at any instant will always be a finite set. Nor can there be any logical objection; logically we could take any initial probabilities we like. The need for such an ordering of laws also corresponds to scientific procedure; we do proceed by testing laws in a special order, that of simplicity, and if we find a satisfactory one we go no further. Anyhow this part of the problem, if we are to get definite answers and not restrict ourselves to general principles, requires that we must find a way of specifying the order of decreasing initial probabilities and saying how fast the series converges.

Wrinch and I were interested primarily in the laws of classical physics, and pointed out one possible rule. If all laws are expressible by differential equations with integral coefficients, the complexity might be defined as the sum of the order, the degree, and the absolute values of the coefficients; and if the complexity so defined is m we could distribute initial probability 2^{-m} equally among all laws of complexity m. This does not cover the whole range of scientific laws; it was intended to dispose of the repeated statement that simplicity is incapable of satisfactory definition. To decide on an order, once the laws are stated, is a trivial matter. But to state all possible laws now would baffle scientific imagination. But even with an incomplete list the method would at any stage attach probabilities to all laws that have been suggested; if new ones are suggested we can consistently revise the postulates to meet the new situation. The immediate effect of revision for well-supported laws is covered by the two general theorems, and will in general be small. The important case is where observation contradicts *all* laws hitherto suggested. Such cases, however, have a remarkably stimulating effect on the scientific imagination.

Bayes and Laplace had proposed uniform distributions of initial probability for unknown parameters in scientific laws. Up to 1918 many philosophers supposed that induction could be adequately described on these lines (though this had already been shown to be too simple a statement by Karl Pearson in 1892). In 1918 it became perfectly clear that it could not; and in 1921 it was clear that a simple modification would give results in accordance with scientific procedure. But many writers still seem to think that if we have a theory of probability at all the uniform distribution of initial probability is an epistemological necessity; Eddington's epistemology contains nothing worse.

Laws usually contain parameters that have to be adjusted as well as possible to fit the observations. Wrinch and I[1] showed in 1919 that in the estimation of a chance, where the possible values form a continuous set the precise form of the prior probability distribution taken for it has very little effect on the posterior probability, and consequently quite crude forms are quite good enough. This can be extended to most estimation problems. For a law containing continuous parameters, if we want to state the results more precisely, we must distribute the initial probability of the law continuously over the possible values. Here we come on another type of problem. We can specify the density ρ of a liquid as the mass per unit volume, but it is completely equivalent to give the specific volume v, the volume per unit mass, and to say that we do not know the density is also to say that we do not know the specific volume. But differences of v are not proportional to differences of ρ, and the uniform distribution of prior probability cannot be right for both. One might think that the solution was obvious when the problem was stated ; I gave it, for the standard error of an observation in the normal law of error, in *Scientific Inference* in 1931. If $P(d\rho|H) \propto d\rho/\rho$, and $v = \kappa\rho^n$, where κ and n are any constants, then $P(dv|H) \propto dv/v$; and this rule is the only one that possesses this property. It leads to a divergent integral if ρ can have any value from 0 to ∞, but this difficulty can be overcome. The rule expresses ignorance of a parameter whenever it would be equally natural to express the law in terms of some power of the parameter. But I have seen four books published since 1945 stating flatly that no way out of the difficulty had ever been suggested. They all quote my *Theory of Probability*, three chapters of which are largely devoted to applications of the rule.

This principle of invariance where a law can be stated in several different forms is capable of extension. We want the probability that the parameters lie in a given region to be the same as the probability that they lie in the corresponding region when they are transformed. For instance, in the Pearson Type VII law

$$P(dx|\lambda, \sigma, m, H) \;\propto\; \left\{1 + \frac{(x-\lambda)^2}{2m\sigma^2}\right\}^{-m} dx$$

we could define $\sigma' = M(m)\sigma$, where M is any function of m, and we want a general rule that will say the same thing in whatever language it is expressed. In this case it can certainly be done. The idea is that we can divide up the range of x into as small intervals as we like. Suppose that on one set of parameters $\alpha_1, \alpha_2 \ldots \alpha_m$ the chance of x occurring in the rth interval is p_r. With a different set of values of parameters the chance is p_r'. Take the sum

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[1] *Phil. Mag.*, 1919, **38**, 715-531

$$J = \Sigma\,(p_r' - p_r) \log \frac{p_r'}{p_r}.$$

If the intervals are arbitrarily small J becomes an integral. It has the essential properties of the square of a distance in Riemannian geometry. It is invariant for all functional transformations of the parameters. Now for small changes of the α, J in general takes the form

$$J = \sum_i \sum_k g_{ik} d\alpha_i d\alpha_k + O(d\alpha^3)$$

and the element

$$\|g_{ik}\|^{\frac{1}{2}}\, d\alpha_1 \ldots\ldots d\alpha_m$$

is invariant for all transformations of the α. It can therefore be consistently used as an element of initial probability in estimation problems.

At this point I should point out a feature in my writings that may have confused some readers. Up to *Scientific Inference* I was interested almost entirely in general principles and worked out definite results for only a few of the simpler cases of estimation. In the early 1930s I was working mainly on the discussion of vast numbers of seismological observations, and the problem of deciding when to alter the form of a law became acute. The previous considerations were useless because I did not know the complexity of the laws in the sense defined, and I was sure that to find it out, even if it was within my mathematical powers, would delay the work by far more than could be compensated by any likely improvement in accuracy. So I developed significance tests on a new principle more directly related to physicists' (especially astronomers' and meteorologists') actual procedure. This is that a change in the form of a law usually consists in practice in the introduction of a new parameter which had previously been tacitly given a special value, but was now regarded as having to be estimated from the observations. The complexity of a law is then redefined as the number of adjustable parameters in it. The number of laws of given complexity is hard to assess, but this hardly affects the results, for previous reasons. If the parameter α_m is zero on the null hypothesis q and we want to test the alternative hypothesis q' that α_m has to be estimated, we can take

$$P(d\alpha_m \mid q'H) = f(J)\, g_{mm}^{\frac{1}{2}} d\alpha_m$$

where J is taken for the comparison of any value of α_m with $\alpha_m = 0$, keeping $\alpha_1 \; . \; . \; . \; . \; \alpha_{m-1}$ the same. $f(J)$ has to be adjusted to make the integral of this expression unity.

In most cases the results are of very similar form when the number of observations, n, is large. If the straightforward estimate of α_m, apart from the significance question, would be $a_m \pm s_m$, we usually get (θ standing for the data collectively)

$$K = \frac{P(q|\theta H)}{P(q'|\theta H)} \doteqdot \frac{A_n^{\frac{1}{2}}}{f(0)} \exp\left(-\frac{a_m^2}{2s_m^2}\right).$$

A is a constant of order 1. We must have $f(0) > 0$, otherwise the null hypothesis would always be asserted. If $f(0) > 0$ and $|a_m| < s_m$, K is large and q has a high probability. If $|a_m|$ greatly exceeds s_m, K is small and q' has a high probability in comparison with q. In practice s_m^2 usually decreases with n like $1/n$, and $K = 1$ for a moderate value of $|a_m|/s_m$, usually 2 to 4.

The idea of invariance therefore goes a long way towards a general quantitative theory. I doubt myself whether it has yet been stated in the best possible form. For one thing, in some simple applications simpler methods lead directly to satisfactory results, whereas the methods based on J need some rather artificial-looking modifications. For another, in some problems the determinant $||g_{ik}||$ does not exist. But other invariants exist, and I think that fuller investigation of the possibilities of invariance theory would be worth the attention of workers in abstract spaces and differential geometry. Dr V. S. Huzurbazar[1] has shown that for those scientific laws that possess what are called ' sufficient statistics ' another type of invariance is possible and in some respects more elegant. It remains to be seen whether his methods can be generalised. (A fuller statement is in Appendix I.)

The whole method depends on our being able to ask our questions in a definite order. This is certainly possible for quantitative laws found by making the parameters adjustable in turn. Essentially it amounts to considering first the hypothesis that all the variation is random, and then testing whether the data support the hypothesis that part of it is calculable for each observation separately. But in its simplest form the problem arises for the identification of a plant by means of an analytical key. Given a specimen, we ask questions in turn, the possible replies at each stage being usually ' yes ' or ' no ', but sometimes several alternatives are possible. At a certain stage we

[1] Ph. D. Thesis, Cambridge, 1949 (unpublished)

are told that the plant is identified. This means, I think, that another specimen similarly identified would be reasonably expected to agree with the first in certain other properties and that seeds from the plant would grow into plants resembling their parent in these properties. It seems to me that the epistemology of this process has been unduly neglected.[1] It is of far wider application than Laplacian induction, and the principles involved in framing and arranging the questions seem to me the sort of thing that philosophers might have something to say about.

I have spoken so far entirely about the process of establishing scientific laws and estimating the parameters in them. Probability is a purely epistemological notion. For something over one hundred years, however, people have tried to define probability in terms of some notion of limiting frequency in an infinite series. There are two objections to this. First, even if such a definition could be given, the epistemological problem would be completely untouched. Secondly, even if the limiting frequency in an infinite series was known, we could draw no conclusions whatever about any finite set without some further principle, which cannot be contained in either pure logic or experience ; and all applications in practice are to finite sets. Wrinch and I said this in 1919, and no reply to our criticism has been given (though there have been replies to other criticisms that we did not make). But since then frequency definitions have been pushed into university and even school courses. I am glad to see that Bertrand Russell and Carnap have made the first point and Littlewood the second ; perhaps now somebody will notice it.

Russell has argued that the theory of probability in its present form still does not justify induction. If in certain circumstances we can say ' it is reasonable to expect on data θH, to the degree indicated by probability 0.9, that p will happen ' we are not saying anything about whether p will in fact happen. He proposes to add some supplementary hypothesis, such as that in 0.9 of such cases in the long run the events predicted with probability p will happen. I cannot accept the extra hypothesis because it sacrifices the applications to separate cases. But the criticism lies deep. I think that it is essentially similar to Lewis Carroll's in ' What the Tortoise said to Achilles '. He discussed a case where we have propositions of the forms p and (p implies q). We want to assert q by itself. This apparently requires a rule ' If p, and p implies q, then we can assert q '. But even if we have this, and *p*, and (*p* implies *q*) we still have not asserted *q*. Trying to state the principle required leads to longer and longer statements that never lead to the desired result. *q* can be detached and asserted by itself only by a rule of procedure, which can be understood and acted on, but cannot be stated in the language of the logical system itself. This being so for pure deduction, it must have a probability analogue, and I think that Russell has shown where it appears : just as in pure logic, we need a rule in probability theory for detaching the consequent, which can not be stated in probability language but can nevertheless be acted on.

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[1] Keynes, I think, went completely astray about it in *Treatise on Probability*, 1921, p. 44. Cf. H. Jeffreys, *Proc. Roy. Soc.* A. 1933, **140**, 528–529 ; 1934, **146**, 9–16.

I shall close with a few remarks on the position of probability in quantum theory. In the first place, if we stick to the purely epistemological view of probability, the apparent irreducible uncertainty of observables in quantum theory was nothing new. Everybody that had handled actual observations knew that they had a scatter, and since Laplace there have been ways of dealing with it. The actual scatter is always far more than the minimum predicted by quantum theory. On the epistemological view of probability, therefore, the uncertainty principle in quantum theory is an ordinary scientific advance that enables us to state some probabilities more precisely.

On the other hand, some scientific laws may contain an element of probability that is intrinsic to the system and has nothing to do with our knowledge of it. Carnap in particular has maintained that there are two kinds of probability. I am inclined to think that there may be such a thing as intrinsic probability, without accepting Carnap's optimistic attempt at a frequency definition of it. Whether there is or not, it can be discussed in the language of epistemological probability. If we accept the notion of intrinsic probability, however, we have a pigeon-hole where we can put any differences there may be between classical determinism and quantum indeterminacy.

Even in this special field, however, I think that the distinction is not very important ; certainly its importance has been exaggerated. Heisenberg did show that conjugate dynamical variables cannot be simultaneously measured to within specified accuracies, but illegitimate conclusions have been drawn. One is that it is meaningless to speak of a simultaneous probability distribution for conjugate variables. To predict the motion of a planet we need the position and velocity at a given instant, and their uncertainties contribute to those of all predictions. If it was meaningless to have a simultaneous probability distribution all classical mechanics would become meaningless. There is no hard and fast boundary between quantum mechanics and classical mechanics; proofs have been given, in some types of problem, that classical mechanics is a limiting case of quantum mechanics when the energy is large.

If we can judge by one problem that has been discussed in detail by wave mechanics, even more is true. C. G. Darwin[1] solved the problem of an electron moving in a uniform field, given a determination of its position with standard error σ at time $t = 0$. His result for time t can be written

$$P(dx\,|\,Ht) \propto \exp\left[-\frac{(x - x_0 - vt - \frac{1}{2}gt^2)^2}{2\{\sigma^2 + (ht/4\pi m\sigma)^2\}}\right] dx.$$

This amounts to saying that the probability distribution for the position at time t is exactly the same as it would be in classical mechanics, given that the joint probability distribution of x and $\dot{x}$ at $t = 0$ is

$$P(dxd\dot{x}\,|\,H, t = 0) = \frac{1}{2\pi h/4\pi m} \exp\left\{-\frac{(x - x_0)^2}{2\sigma^2}\right\} \exp\left\{-\frac{(\dot{x} - v)^2}{2(h/4\pi m\sigma)^2}\right\} dxd\dot{x}$$

and that the acceleration is always g. But this is as complete determinism as was ever supposed in classical mechanics. Not only does the existence of a joint probability distribution lead to the quantum theory answer, but every element of it, however small, at time 0 transforms into a corresponding element at time t. Thus, though there is a limit to the accuracy with which x and $\dot{x}$ can be measured, we can *speak* of them to any accuracy we like and get the right answer. I strongly suspect that physicists' statements on this point represent a flight from idealist criticism, and it is perfectly needless. A realist may admit that many statements have meanings even though he may not have verified their truth in every detail or even be able to do so. In predicting the motion of a planet we do not verify the position corresponding to every intermediate step in the calculation; nor do we abandon the attempt even if cloudiness makes observations impossible at some of the times considered. Scientists do habitually speak in terms of realism. I am not saying that idealism is false; but I do say that it can have no claim to be considered as a serious contribution to scientific method until it develops its language to a point where it can give an adequate account of these ordinary processes of calculation.

[1] C. G. Darwin, *Proc. Roy. Soc.* A, 1927, **117**, 273

Lest the Darwin electron should be regarded as an artificial example, I should point out that the simultaneous determination of the co-ordinate and momentum of an electron was one of Heisenberg's fundamental illustrative experiments. The probability given is for times when the electron is not observed, since by hypothesis the only observation is at $t = 0$ and the verifiability criterion is not satisfied. Moyal and Bartlett[1] have given joint probability distributions for co-ordinates and momenta in several of the standard problems of atomic physics. In most of these classical determinism does not hold ; change of t does not transform points into points.

I think that the best evidence for an intrinsic probability in quantum theory is to be found in other experiments. It has long been known that if the α-particles emitted by a specimen of a radio-active substance in successive equal intervals of time are counted, the numbers are consistent with their having been derived independently from a Poisson law of chance. The stress is on independence ; given the chance of an emission in one second, the chance in any other second is the same whatever may have happened before. I know of no corresponding data for the photo-electric effect, but I think that if they were not similar every physicist would know about it. Theoretically the emission of radiation from an illuminated gas would have similar properties, but in laboratory conditions it is usually over too soon for the counts to be made. To take another illustration from radio-activity, suppose that we have a radio-active substance with an average decay time of a few minutes, and that the next in its series has one of a few days. Collect the degeneration products of the first in a minute, and watch what happens to them. On a determinist theory, since the most striking feature in their recent history is the disruption that produced them, we should expect that they would all break up at a fixed time after their formation, and therefore within a minute of one another. This could hardly have failed to be noticed.

APPENDIX I

A summary of the properties of certain invariants may be useful.

1. $$J = \Sigma_r \, (p_r' - p_r) \log \, (p_r'/p_r).$$

For continuous distributions, if p_r is replaced by dP,

$$J = \int \log \, (dP'/dP) \, d(P' - P).$$

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[1] J. E. Moyal, *Proc. Camb. Phil. Soc.*, 1949, **45**, 99-124 ; J. E. Moyal and M. S. Bartlett, *Proc. Camb. Phil. Soc.*, 1949, **45**, 545-553

In general J is quadratic for small changes of the parameters and leads to an in variant differential form $\|\, g_{ik} \,\|^{\frac{1}{2}} \Pi d\alpha_m$. For comparisons of joint chances of n observations derived from two laws, J is n times the J for comparison of chances for one observation from each law. This suggests an extension to cases where the chance of an observed value depends on the results of previous observations (stochastic processes). But for comparison of two rectangular laws J is infinite. For the significance test for a new location parameter α_m a factor $(1/\pi)\, d \tan^{-1} J^{\frac{1}{2}}$ has to be inserted in the prior probability. This rule can be generalised, but where the chance on the null hypothesis is 0 or 1 it makes J infinite for all other values and the alternative hypothesis becomes identical with the null hypothesis.

In view of the fact that laws must be tested in a special order, at any stage the prior probability of the parameters present on the null hypothesis is given by previous significance tests, and the extreme generality given by J is not required.

2. $I_2 = \int \{(dP')^{\frac{1}{2}} - (dP)^{\frac{1}{2}}\}^2$. For small changes $I_2 \doteq \frac{1}{4} J$ in general and leads to the same estimation rule. It also has an invariance property for the joint chance of n observations and can be adapted to significance tests, but in most cases the form is more complicated. It does not break down in testing whether a simple chance is 0 or 1, but in comparison of two rectangular laws I_2 is of the first order in their difference.

3. $I_1 = \int |dP' - dP|$. Has not been investigated in detail, but is in general linear for small changes in the parameters, including some cases where J and I_2 are not quadratic. Much more complicated than J or I_2 in most cases, but simpler for comparison of two simple chances. Leads to no simple rule for joint probability distributions for several parameters, but in view of the above remarks on significance tests this may be unimportant.

I_1 could be adapted to give a function with an additive property. If $\alpha < \alpha' < \alpha''$, we can define

$$I_1(\alpha, \alpha + \delta\alpha) = L(\alpha)\delta\alpha + o(\delta\alpha),$$

and then L has the property that

$$\int_{\alpha}^{\alpha''} L d\alpha = \int_{\alpha}^{\alpha'} L d\alpha + \int_{\alpha'}^{\alpha''} L d\alpha.$$

L is usually simpler in form than I_1.

4. If $P(x \mid \alpha H) = 2^{-n}$ for $x = n\alpha$, and otherwise is 0, all the above invariants take their maximum values for comparison of incommensurable α, α', however small $| \alpha' - \alpha |$ may be. The invariant $| P' - P |_{\max}$ is free from this defect and in other respects is similar to I_1. But it is immediately applicable only to laws that contain only one variable.

5. Sufficient statistics. According to a theorem of Pitman and Koopman, if a law of chance has the form

$$P(dx_1dx_2 \, . \, . \, dx_k \mid \alpha_1 \, . \, . \, . \, . \, \alpha_m H) = \phi(\alpha)\psi(x) \exp \Sigma \, u_r(\alpha)v_r(x)$$

the functions $Sv_r(x)$ over a set of observations contain all the information in the observations that is relevant to the α_i. The converse, that the existence of such functions implies this form of the law, has been proved in general subject to certain conditions of differentiability, but is in fact true in all known cases where such functions exist, including some that do not satisfy the stated condition of differentiability. u_r, v_r may be functions of all the αs and xs respectively.

Huzurbazar points out that in such cases $u_r(\alpha)$ could be taken as a new parameter β_r, and that with any transformation of the α_r the β_r undergo only linear transformations. By a linear transformation of β_r the possible values of each β_r can be made to cover one and only one of the ranges $(-\infty, \infty)$, $(0, \infty)$, $(0, 1)$. An alternative definition of the parameters is to take the expectations of $v_r(x)$, which have similar properties. Then in the three cases the prior probabilities of β in estimation problems may be taken distributed as $d\beta$, $d\beta/\beta$, or $d\beta$.

The chief interest of the results is that they give a clear classification of types of law, and some such classification may be needed in any case. The rules have not been extended to significance tests, and it is not known whether they can be modified so as to apply to laws that do not yield sufficient statistics.

APPENDIX II

Initial probabilities of laws

Another type of generalisation that needs attention is as follows. If we are making a single drawing of a ball from a bag containing two balls, the probability that the ball will be white may be $\frac{1}{2}$ if either (1) the bag is known to contain one white and one black ball; (2) the balls are known either to be both white or both black, but we do not know which; or (3) the balls are equally likely to be both white, both black or one white and one black. Suppose we draw a ball and it is white. What is the probability that the other is white? In the three cases the answers are 0, 1, $\frac{2}{3}$.

In stating initial probabilities for laws we are never asserting that we know how frequent instances of particular laws are in the world, nor that all events are covered by the same type of law. Thus (1) and (2) are not suitable analogues; the proper analogue is (3).

It is still often argued that induction is justified by its success in previous instances, and nearly as often pointed out that such an argument can lead only to an infinite regress. But it can be argued that the establishment of a law of given form in many cases implies a strong probability that a law of that form will hold in analogous cases, and that prior probabilities need revision from time to time to allow for this. Nobody hesitates to apply the law of gravitation to a new planet or comet.

The following drastically simplified statement is intended only for illustration. If the possible laws were finite in number and their chances $p_1 \ldots p_n$, and the joint probability distribution of these chances was

$$P(dp_1 dp_2 \,.\,.\, dp_{n-1} \mid H_0) \propto dp_1 dp_2 \,.\,.\, dp_{n-1},$$

and if the rth law has been established in x_r cases, it is known that the probability that the rth law will hold in the next case is $(x_r + 1)/(\sum_{s=1}^{n} x_s + n)$, irrespective of the values of x_s other than for $s = r$, subject to their sum remaining the same. (This comes from a straightforward extension of Laplace's theory; the complication that the laws are not conclusively established is ignored for simplicity.) This would be the proper value to take for the prior probability in a new investigation.

If, however, the joint probability is not uniform, say

$$P(dp_1 \ldots dp_{n-1} \mid H_0) \propto f(p_1, p_2 \,.\,.\, p_{n-1}) dp_1 dp_2 \,.\,.\, dp_{n-1}$$

with f not constant, it is easy to show that not only does the result not hold, but that the probability that the rth law will hold in the next instance does not depend only on x_r and $\sum x_s$. This can be seen without detailed analysis. Suppose that only three laws were concerned and that f is large for $p_1 = p_2 \neq p_3$ and zero for $p_1 = p_3$. If $x_2 = a > x_1$ and is large, and $x_3 = 0$, p_2 will probably be near $a/(a + x_1)$, and then f is large for p_1 near $a/(a + x_1)$. If, however $x_2 = 0$, $x_3 = a$, p_2 will probably be small, p_3 near $a/(a + x_1)$. Then f is small for p_1 near $a/(a + x_1)$ and large for p_1 small. Thus in such a case the form of f will lead to a larger probability for the first law in the next trial if p_2 has been often verified than if p_3 has been equally often verified.

This situation might be important theoretically in testing laws that have few or no previous instances. Its practical importance is likely to be small, but there is a suggestion that laws may fall into groups whose probabilities in many circumstances will rise in probability simultaneously. This could correspond to a null hypothesis and a group of hypotheses derived from it by making the parameters adjustable. Exclusion of widely different laws would affect the probabilities of all these alike, and discrimination between them would only arise when specific significance tests are applied.

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ON THE NOTION OF INVARIANCE IN CLASSICAL MECHANICS *

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1 *Introduction*

The notion of invariance is widely used in mathematics, but the theory of invariance (in a given mathematical domain) is not ordinarily presented in a logically clear way. Usually there is not even given a reasonably precise characterisation of the class of mathematical entities whose invariance is to be studied. The notion of invariance is also important in theoretical physics, and has been much discussed since the advent of relativity theory. In physics, even more than in mathematics, a bewildering array of entities is classified as invariant or not invariant. Thus it is common to say that there is no satisfactory Lorentz invariant notion of centre of mass, or that the equation expressing Newton's second law of motion is Galilean invariant. In the one case the existence of a certain kind of mathematical entity—a function with certain specified properties—is denied, and in the other case a linguistic entity—a certain equation—is said to have an invariance property.

Just as confusing are some of the strange definitions of invariance. For instance, in a recent issue of this *Journal* C. W. Blair[1] approvingly quotes the following :[2]

> By invariance, we mean the constancy of some complex entity or object of thought while certain of its constituent elements or associated quantities undergo change.

From a formal standpoint this definition is quite inadequate : it is framed in a psychological terminology which is inappropriate in

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* Received 5. xi. 53

† It is with regret that we record the death of Professor McKinsey. John Charles Chenoweth McKinsey was born in Frankfort, Indiana, in 1908. He had a very distinguished career and held the post of professor of philosophy at Stanford University when he died. He contributed many important papers on mathematics and logic. The present one was the last piece of work he was engaged upon.

[1] C.W. Blair, ' Some aspects of the search for invariants ', this *Journal*, 1950, 1, 230

[2] H. V. Craig, *Vector and Tensor Analysis*, New York, 1943, p. 7

physics; and in addition involves the obviously vague notions of constancy and change in time.[1]

In the present paper we do not attempt a rational reconstruction of the notion of invariance *in general.* Our aim is to illustrate by the example of Galilean invariance in classical particle mechanics what seems to be a general method of giving such an explication in particular cases. We have chosen a domain of physics rather than of mathematics, such as topology, for several reasons. In the first place, there is greater need for clarification of the notion of invariance in physics because usage there is more confused than in mathematics. For instance, the dictum of the mathematicians—that an entity is topologically invariant if it is invariant under one-to-one bi-continuous transformations—appears much sharper than the corresponding dictum of the physicists about invariance in classical mechanics. This difference in sharpness is due to the fact that mathematicians know pretty well what they mean by a topological space, and when they speak of transformations they are referring to transformations which carry one topological space into another. Physicists, on the other hand, have not reached an equal degree of clarity as to what they mean by a system of classical mechanics. To achieve reasonable clarity, we must restrict our discussion to branches of physics which have been axiomatised in a precise way. In this paper we confine our attention to classical particle mechanics,[2] since this branch of physics is at once familiar, and relatively simple in its mathematical structure. A very similar discussion, however, could be given for relativistic particle mechanics,[3] and for classical rigid body mechanics.[4] On the other hand, we would not be prepared to discuss the notion of invariance in thermodynamics, for instance, because so far as we know this discipline has not yet been axiomatised in a sufficiently precise way.

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[1] The usual procedure in the more carefully written mathematical treatises is to give an intuitive discussion of invariance, but to avoid explicit definition and use of the notion in formulating theorems. See, e.g. M. H. Stone, *Linear Transformations in Hilbert Space*, New York, 1932, pp. 83-85.

[2] The axiomatisation for classical particle mechanics of which we shall make use is a trivial modification of one given by J. C. C. McKinsey, A. C. Sugar and P. Suppes, 'Axiomatic foundations of classical particle mechanics', *Journal of Rational Mechanics and Analysis*, 1953, 2, 253.

[3] An axiomatisation of relativistic particle mechanics is given in H. Rubin and P. Suppes, 'Transformations of systems of relativistic particle mechanics', to appear in the *Pacific Journal of Mathematics.*

[4] An axiomatisation of classical rigid body mechanics, due to Ernest Adams and Herman Rubin, is to be given in E. Adams, 'Axiomatic foundations of classical rigid body mechanics', Dissertation, 1954, Stanford University, Stanford, California.

Another reason for choosing a domain of physics rather than a branch of mathematics is that the explication of the notion of invariance for such a domain seems to have relevance to problems of empirical or physical meaningfulness, as discussed by philosophers and physicists in recent years. This question is considered in Section 4.

For an adequate explication of the notion of invariance for a given domain—classical particle mechanics in this instance—three essential questions need to be answered, and the attempt to answer them will be our main concern.

First, what sort of entities are to be conceived of as being transformed ? Intuitively, the proper answer seems to be : *systems* of classical particle mechanics. A formal definition of the notion of such systems is given in the next section.

Secondly, what is the set (usually a group) of transformations being considered ? A natural answer to this question is given in Section 2, and an alternative is suggested in Section 5.

Thirdly, what sort of entities are to be classified as invariant or not invariant ? This question is treated in Section 3.

We remark that throughout this paper we use set-theoretical rather than meta-mathematical methods. This choice has not been a capricious one on our part, for, in our opinion, meta-mathematical methods have not yet been successfully applied to any branch of science—such as mechanics, optics, or thermodynamics—involving substantial portions of classical analysis. We believe, moreover, that the difficulties involved in applying meta-mathematical methods to such disciplines are formidable.[1] In order to make such an application, the formal language developed would have to be rich enough to contain not merely the elementary arithmetic to be expected in studies of the foundations of mathematics, but also such standard mathematical disciplines as matrix theory, the differential and integral calculus, and at least a good part of the theory of functions of a real variable.

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[1] In fact, in our opinion, the emphasis by contemporary philosophers on the linguistic or semantical approach is generally responsible for the lack of substantial progress in the philosophy of science. Most of the precise discussions have been restricted to languages with qualitative predicates only, and consequently to languages which are wholly inadequate to any complicated branch of science. Even a subject as relatively uncomplicated as the axiomatic foundations of the theory of measurement must use the notions and results of elementary mathematical analysis, which is a discipline extremely tedious to handle in a formalised language.

From a formal standpoint, the discussion of physical notions and concepts by physicists is ordinarily an extremely confusing mixture of the set-theoretical and linguistic viewpoints.[1] This mixture can probably be untangled only by adopting unequivocally one of the two viewpoints ; and for the reasons already stated, we adopt the former.

2 *Axioms and Preliminary Notions*

By and large our mathematical notation is standard. We explain here some of the less familiar notations used. We use the letter 'N' to denote the set of all positive integers. If f is a one-to-one function then f^{-1} is the *inverse* of f: i.e. for every x in the domain of f, $f^{-1}(f(x)) = x$; and for every y in the range of f, $f(f^{-1}(y)) = y$. If n is a positive integer, $\langle a_1, a_2, \ldots, a_n\rangle$ is the *ordered n-tuple* whose first member is a_1, whose second member is a_2, and so on. By a *vector* we shall always mean here a three-dimensional vector : i.e. an ordered triple of real numbers. If A and B are sets, then $A \times B$, the *cartesian product* of A and B, is the set of all ordered couples $\langle a, b\rangle$, where $a \in A$ and $b \in B$.

For derivatives we use a notation similar to one suggested by Menger.[2] If g is a function of any number of arguments, then Dg is the derivative of g with respect to its *last* argument. Thus if, for all x and y,

$$g(x, y) = x^3 + xy^4$$

then Dg is the function such that, for all x and y,

$$Dg(x, y) = 4xy^3$$

(It happens that we never need to take derivatives except with respect to the last arguments of functions ; hence we need not introduce notation for derivatives with respect to arbitrary arguments.) We usually write 'D^2g' instead of 'DDg'. Thus, with g as above, D^2g is the function such that, for all x and y,

$$D^2g(x, y) = 12xy^2$$

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[1] The same sort of confusion is prevalent in standard works on mathematics. A good example is the usual treatment of polynomials, which are ordinarily described as expressions consisting partly of non-linguistic (!) entities : 'A polynomial is an expression of the form

$$a_0 + a_1x + \ldots + a_nx^n, \text{ where } a_0, a_1, \ldots, a_n \text{ are numbers.'}$$

[2] K. Menger, 'Are variables necessary in calculus ?', *American Mathematical Monthly*, 1949, **56**, 609

To answer the first of our three questions, we now introduce the notion of a system of classical particle mechanics.

Definition 2 (1). Γ is a *system of classical particle mechanics* if, and only if, there exist sets P and T, a function m whose domain of definition is P, a function s whose domain of definition is the cartesian product $P \times T$, and a function f whose domain of definition is the cartesian product $P \times T \times N$, such that $\Gamma = \langle P, T, m, s, f \rangle$ and the following axioms are satisfied :

(i) P is finite and non-empty ;
(ii) T is an interval of real numbers ;
(iii) if p is in P and t is in T, then $s(p, t)$ is a vector, and $D^2 s$ exists at every point of $P \times T$;
(iv) if p is in P, then $m(p)$ is a positive real number ;
(v) if p is in P and t is in T, then $f(p, t, 1), f(p, t, 2), \ldots, f(p, t, i)$, . . . are vectors such that the series

$$\sum_{i=1}^{\infty} f(p, t, i)$$

is absolutely convergent ;

(vi) for all p in P and t in T,

$$m(p) D^2 s(p, t) = \sum_{i=1}^{\infty} f(p, t, i)$$

Remark 2 (2). In the usual physical models of Definition 2 (1), we take P to be a set of *particles*, T to be a set of numbers measuring elapsed *times*, and m, s, and f to be, respectively, the *mass*, *position* and *force* functions : thus if p is in P, then $m(p)$ is the numerical value of the mass of p ; if p is in P and t is in T, then $s(p, t)$ is a vector representing the position of p at time t ; and if p is in P, t is in T, and i is in N then $f(p, t, i)$ is a vector representing the i^{th} force acting on particle p at time t.

We shall denote by '$\mathfrak{S}$' the set of all systems of classical particle mechanics, and by 'V' the union of all the first components of members of $\mathfrak{S}$.[1] As we shall see, the introduction of the set $\mathfrak{S}$ is very useful. It may be of some general interest to note that in special cases one can often avoid difficult meta-mathematical notions, such as the notion of logical consequence, by considering the set of all models of the axiomatisation being investigated. A simple example from Boolean algebra may suffice to illustrate this point. If we define a Boolean algebra as a system $\langle K, +, \cdot\rangle$ satisfying certain axioms, then we can replace the meta-mathematical theorem :

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[1] It might appear at first blush that the assumption that such a set $\mathfrak{S}$ exists would lead to a logical contradiction. Our discussion can easily be modified, however, so as to allay such misgivings : thus we could consider only systems $\langle P, T, m, s, f \rangle$ of classical particle mechanics where P was a subset of some fixed infinite set A (in which case, of course, we should have $V = A$). In this connection, see the remarks in Appendix II of J. C. C. McKinsey and A. Tarski, 'The Algebra of topology', *Annals of Mathematics*, 1944, 45, 141.

> The statement ' For every x in K, $x + x = x$ ' is a logical consequence of the axioms of Boolean algebra,

by the set-theoretical theorem :

> For every Boolean algebra $\langle K, +, \cdot\rangle$, if x is any element of K, then $x + x = x$.

And by introducing the set $\mathfrak{B}$ of all Boolean algebras, one can define a mathematical correlate, for example, of such a more general meta-mathematical property as an equation's being a logical consequence of the axioms of Boolean algebra. In an analogous way, in the next section we shall use the set $\mathfrak{S}$ of all systems of classical particle mechanics in order to avoid meta-mathematical constructions.

We next introduce the notion of a Galilean carrier. Intuitively, a Galilean carrier corresponds to a transformation of a system of particle mechanics from one inertial frame to another, where the units of measurement remain unchanged. The nature of this correspondence will be made clear in Definition 2 (5).

Definition 2 (3). A *Galilean carrier* is an ordered sextuple $\langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha\rangle$, where ϕ_1 is a one-to-one mapping of V on to itself, ϕ_2 is a one-to-one mapping of N on to itself, $\mathfrak{A}$ is an orthogonal 3 by 3 matrix, B and C are vectors, and α is a real number.

The proof of the following theorem is almost immediate.

Theorem 2 (4). Let $\Gamma = \langle P, T, m, s, f\rangle$ be a member of $\mathfrak{S}$ (i.e. a system of classical particle mechanics) ; let $\Delta = \langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha\rangle$ be a Galilean carrier ; let P' be the set of all elements p' of V such that for some p in P,

$$p' = \phi_1(p) \, ;$$

let T' be the set of all real numbers t' such that, for some t in T,

$$t' = \alpha + t \, ;$$

and let the functions m', s', and f' be defined by the equations (for p'

any element of P', t' any element of T', and i any element of N) :

$$\begin{aligned} m'(p') &= m[\phi_1^{-1}(p')] \\ s'(p', t') &= s[\phi_1^{-1}(p'), t' - \alpha] \cdot \mathfrak{A} + t' B + C \\ f'(p', t', i) &= f[\phi_1^{-1}(p'), t' - \alpha, \phi_2^{-1}(i)] \cdot \mathfrak{A} \end{aligned}$$

Then $\Gamma' = \langle P', T', m', s', f' \rangle$ is also a member of $\mathfrak{S}$.

The intended intuitive interpretation of the components of a Galilean carrier $\langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha \rangle$ should be clear from the above theorem. ϕ_1 replaces the particles of a system by new particles with similar mechanical properties ; the introduction of this function is justified in Remark 3 (5). ϕ_2 renumbers the given forces. The multiplication of $s[\phi_1^{-1}(p'), t' - \alpha]$ by the orthogonal matrix $\mathfrak{A}$ amounts to a rotation, and possibly a reflection, of the co-ordinate system. The vectors C and $t'B$ referred to in the equation defining s' amount respectively to a translation, and the imposition of a uniform velocity for the new co-ordinate system with respect to the old. The equation

$$t' = \alpha + t$$

shifts the origin of time by an amount $-\alpha$.

Definition 2 (5). If Δ is a Galilean carrier, then a function Φ is *the Galilean transformation corresponding to* Δ if, and only if, for every Γ and Γ' in $\mathfrak{S}$,

$$\Phi(\Gamma) = \Gamma'$$

when, and only when, Δ, Γ, and Γ' are related as in Theorem 2 (4). Moreover, a function Φ is called simply a *Galilean transformation* if it is the Galilean transformation corresponding to some carrier Δ.

From Theorem 2 (4) it is seen that the domain of definition of every Galilean transformation is $\mathfrak{S}$; in addition, every such transformation is a one-to-one mapping of $\mathfrak{S}$ on to itself.

It is readily verified that the set of all Galilean transformations is a group. The well-known physical significance of this group leads us to propose it as an answer to the second question raised in Section 1.

3 *Invariant Notions*

In the previous section we have answered the first two questions raised in the Introduction : (1) we have defined systems of particle mechanics, which are the entities to be transformed ; and (2) we have defined the Galilean group of transformations with respect to which it is physically reasonable to require invariance. We now answer the third question, by presenting a definition of SCPM (System-of-

Classical-Particle-Mechanics)—functions, which are the entities to be classified as invariant or not invariant. Roughly speaking, an SCPM-function is a function the domain of whose first argument is the set $\mathfrak{S}$, and whose remaining arguments are particles or instants of time. The SCPM-functions which are of interest correspond to various standard mechanical notions, such as mass, acceleration, and angular momentum.

Definition 3 (1). Let u and v be non-negative integers. Then a function f is an SCPM-*function* (*of type* $\langle u, v\rangle$) if, and only if, the domain of definition of f consists of all $(u + v + 1)$-tuples $\langle \Gamma, p_1, \ldots, p_u, t_1, \ldots, t_v\rangle$, where $\Gamma = \langle P, T, m, s, f\rangle$ is a system of classical particle mechanics, and p_i belongs to P and t_j belongs to T, for $i = 1, \ldots, u$, and $j = 1, \ldots, v$.

Remark 3 (2). In the special case where u (or v) is zero, an SCPM-function has no particles (no instants of time) as arguments. Thus, for example, an SCPM-function of type $\langle 0, 1\rangle$ is a function whose domain of definition consists of all couples $\langle \Gamma, t\rangle$, where $\Gamma = \langle P, T, m, s, f\rangle$ is a system of classical particle mechanics and t belongs to T.

The reader may wonder why it is necessary, in the definition of an SCPM-function, to include Γ as the first argument. This is due to the fact that the same real number can occur in the second component (interpreted as measures of elapsed times) of many different systems of particle mechanics; and similarly for the particles. Thus, for example, we can have two systems $\Gamma = \langle P, T, m, s, f\rangle$ and $\Gamma = \langle P', T', m', s', f'\rangle$ of classical particle mechanics, and an entity p which belongs both to P and to P', and such that $m(p) \neq m'(p)$.

It may be felt that the formally complicated notion of an SCPM-function does not correspond to anything involved in the intuitive discussions by physicists of physical notions, and that accordingly we have chosen the wrong set of entities to classify with respect to invariance. However, we see no simpler way of making a rational reconstruction which avoids a confused mixture of linguistic and set-theoretical discourse.

Remark 3 (3). It will be noticed that we have made no restriction on the range of values of an SCPM-function. Thus the values of such a function may be real numbers, vectors, matrices, etc. In view of this generality, we may in particular regard a relation as a special kind of function, whose values are always either 0 or 1: we conceive of the relation as holding when, and only when, the function has the value 1.

Definition 3 (4). An SCPM-function f of type $\langle u, v\rangle$ is *Galilean invariant* if, and only if, for every Galilean carrier $\Delta = \langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha\rangle$, with Φ the corresponding Galilean transformation, for every $\Gamma = \langle P, T, m, s, f\rangle$ in $\mathfrak{S}$, for $p_1, \ldots, p_u$ any elements of P, and for $t_1, \ldots, t_v$ any elements of T,

$$f(\Gamma, p_1, \ldots, p_u, t_1, \ldots, t_v) = f(\Phi(\Gamma), \phi_1(p_1), \ldots, \phi_1(p_u), \alpha + t_1, \ldots, \alpha + t_v)$$

The following are examples of Galilean invariant SCPM-functions :

(1) *Mass* : i.e. the SCPM-function μ_1 of type $\langle 1, 0\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p belongs to P, then

$$\mu_1(\Gamma, p) = m(p)$$

(2) *Distance between particles :* i.e. the SCPM-function μ_2 of type $\langle 2, 1\rangle$ such that, if $\Gamma = \langle P\ T, m, s, f\rangle$ is any member of $\mathfrak{S}$, if p_1 and p_2 belong to p, and if t belongs to T, then

$$\mu_2(\Gamma, p_1, p_2, t) = |\, s(p_1, t) - s(p_2, t)\, |$$

(3) *Magnitude of acceleration :* i.e. the SCPM-function μ_3 of type $\langle 1, 1\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p is in P and t is in T, then

$$\mu_3(\Gamma, p. t) = |\, D^2 s(p, t)\, |$$

On the other hand, the following SCPM-functions are not Galilean invariant :

(4) *Momentum :* i.e. the (vector-valued) SCPM-function μ_4 of type $\langle 1, 1\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p is in P and t is in T, then

$$\mu_4(\Gamma, p, t) = m(p)Ds(p, t)$$

(5) *Centre of mass :* i.e. the SCPM-function μ_5 of type $\langle 0, 1\rangle$, such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if t is any element of T, then

$$\mu_5(\Gamma, t) = \frac{\sum_{p \epsilon P} m(p)s(p, t)}{\sum_{p \epsilon P} m(p)}$$

(6) *Redness :* i.e. the SCPM-function μ_6 of type $\langle 1, 0\rangle$, such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p is in P, then

$$\mu_6(\Gamma, p) = 1, \text{ if } p \text{ is a red object,}$$
$$\mu_6(\Gamma, p) = 0, \text{ otherwise}$$

Remark 3 (5). In connection with the SCPM-function μ_6, we remark that Definition 2 (3) was intentionally framed to keep such non-mechanical properties as redness from being Galilean invariant: the introduction of the function ϕ_1 in Definition 2 (3) accomplishes this end.

We also remark that although μ_4 and μ_5 are not Galilean invariant, these functions enter into mechanically important relations which are Galilean invariant. Thus these functions differ strikingly from such functions as μ_6.

4 *Invariance and Problems of Meaningfulness*[1]

During the past two decades, philosophers have been much concerned with the problem of providing an adequate empiricist criterion of meaning. The main proposals in this connection have recently been reviewed and succinctly criticised by Hempel.[2] It is not our purpose here again to summarise these proposed criteria. The more recent literature on the subject, however, indicates an increasing awareness of the difficulty and complexity of the problem, especially when one attempts to cope with any branch of science which makes extensive use of mathematics: Philosophers now seldom hope to reduce all the terms of such 'advanced' branches of science to qualitative, directly observable predicates; and the tendency is becoming prevalent to regard meaningfulness as a notion applicable only to a scientific theory as a whole.

This recent philosophical literature tends to give the impression that all theoretical concepts which play a proper rôle in a given theory are equally meaningful. However, a vast literature of physics is at variance with this view. For example, in discussing Galileo's principle of relativity, Weyl[3] says:

> Only the motions of bodies (point-masses) relative to one another have an objective meaning,

and, also:[4]

> A relation between world-points has an objective meaning if, and only if, it is defined by such arithmetical relations between the

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[1] We are grateful to Professor Donald Davidson for some helpful suggestions and criticisms in connection with this section.

[2] C. G. Hempel, 'Problems and changes in the empiricist criterion of meaning', *Revue Internationale de Philosophie*, 1950, no. 11, 41; 'The concept of cognitive significance: a reconsideration', *Proceedings of the American Academy of Arts and Sciences*, 1951, **80**, 61

[3] H. Weyl, *Space-Time-Matter*, 4th ed., London, 1922, p. 152

[4] ibid., p. 155

co-ordinates of the points as are invariant with respect to the (Galilean) transformations.

Thus physicists are interested in providing, within a given discipline, an *internal* classification of concepts according to their meaningfulness.

Moreover, in contrast to philosophers, physicists and mathematicians (e.g. Weyl) propose to classify certain *non-linguistic* entities as meaningful or meaningless. Philosophers who find this usage too disturbing would do well to consider, not only the long tradition of such usage in physics, but also the difficulty (mentioned in Section 1) of formalising advanced branches of science.

Using the definitions of the previous section, we are in a position to give a precise formulation of Weyl's intuitive notion of objective meaning: *an SCPM-function is mechanically meaningful only if it is Galilean invariant (in the sense of Definition 3 (4)).*

It may be remarked that the conditions which early logical empiricists wished to impose on meaningfulness were so stringent, that it is doubtful whether those philosophers would have regarded the above criterion as a *sufficient* condition for mechanical meaningfulness. The nature of their objections may be illustrated by the following example:

(7) Let μ_7 be the SCPM-function of type $\langle 1, 0\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p is in P, then

$$\mu_7(\Gamma, p) = 1 \text{ if } \mathrm{m}(p) \text{ is rational,}$$
$$\mu_7(\Gamma, p) = 0 \text{ if } m(p) \text{ is irrational.}$$

The function μ_7 is Galilean invariant, but early logical empiricists would probably have regarded it as empirically meaningless on the ground that no finite set of observations could determine whether $\mu_7(\Gamma, p)$ was 1 or 0.

5 *Generalised Galilean Invariance*

The physicist who is content to accept the criterion of mechanical meaningfulness stated in Section 4 apparently commits himself to an absolute system of units of measurement for mass, time, distance, and force. On the other hand, the physicist or philosopher who regards the adoption of a particular system of units as an arbitrary convention without mechanical significance will probably prefer a criterion in terms of the notion of *generalised Galilean invariance*; the explanation of this notion requires some developments similar to those of Sections 2 and 3.

We begin by generalising the notion of a Galilean carrier. Intuitively, a generalised Galilean carrier corresponds to a transformation of a system of particle mechanics from one inertial frame to another, where the units of measurement may be changed.

Definition 5 (1). *A generalised Galilean carrier* is an ordered 9-tuple $\langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha, \beta, \gamma, \delta\rangle$, where $\langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha\rangle$ is a Galilean carrier, β is a real number different from 0, and γ and δ are positive real numbers.

The following theorem is a generalisation of 2 (4).

Theorem 5 (2). Let $\Gamma = \langle P, T, m, s, f\rangle$ be a member of $\mathfrak{S}$; let $\Delta = \langle \phi_1, \phi_2, \mathfrak{A}, B, C, \alpha, \beta, \gamma, \delta\rangle$ be a generalised Galilean carrier ; let P' be the set of all elements p' of V such that, for some p in P,

$$p' = \phi_1(p)$$

let T' be the set of all real numbers t' such that, for some t in T,

$$t' = \alpha + \beta t$$

and let the functions m', s', and f' be defined by the equations (for p' any element of P', t' any element of T', and i any element of N) :

$$m'(p') = \gamma m[\phi_1^{-1}(p')]$$

$$s'(p', t') = \delta s\left[\phi_1^{-1}(p'), \frac{t' - \alpha}{\beta}\right] \cdot \mathfrak{A} + t' B + C$$

$$f'(p', t', i) = \frac{\gamma\delta}{\beta^2} f\left[\phi_1^{-1}(p'), \frac{t' - \alpha}{\beta}, \phi_2^{-1}(i)\right] \cdot \mathfrak{A}$$

Then $\Gamma' = \langle P', T', m', s', f'\rangle$ is also in $\mathfrak{S}$.

The intended intuitive interpretation of ϕ_1, ϕ_2, $\mathfrak{A}$, B, C, and α has already been given in Section 2. The introduction of β amounts to changing the unit of time in the ratio $1/\beta$; the introduction of γ amounts to changing the unit of mass in the ratio $1/\gamma$; and the introduction of δ amounts to changing the unit of distance in the ratio $1/\delta$. These changes in the units of time, mass and distance, necessitate (in order that Newton's Second Law remain true) that the unit of force be changed in the ratio $\beta^2/\gamma\delta$.

It is now clear that (analogously to 2 (5)) one can define the *generalised Galilean transformation corresponding to a* given generalised Galilean carrier. Moreover, one can define *generalised Galilean invariance* analogously to the definition of Galilean invariance in 3 (4) (in the equation of 3 (4) it is necessary merely to replace '$\alpha + t_1$' by '$\alpha + \beta t_1$', etc.).

It may be noticed that the SCPM-functions μ_1, μ_2, and μ_3

Section 3, although they are Galilean invariant, are not generalised Galilean invariant. However, the following two examples, related to μ_1 and μ_2 are generalised Galilean invariant.

(8) *Ratio of masses:* i.e. the SCPM-function μ_8 of type $\langle 2, 0\rangle$ such that if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p_1 and p_2 are in P, then

$$\mu_8(\Gamma, p_1, p_2) = \frac{m(p_1)}{m(p_2)}$$

(9) *Ratio of distances between particles:* i.e. the SCPM-function μ_9 of type $\langle 4, 1\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p_1, p_2, p_3, and p_4 are in P, and t is in T, then

$$\mu_9(\Gamma, p_1, p_2, p_3, p_4, t) = \frac{|s(p_1, t) - s(p_2, t)|}{|s(p_3, t) - s(p_4, t)|}$$

if $|s(p_3, t) - s(p_4, t)| \neq 0$, and otherwise

$$\mu_9(\Gamma, p_1, p_2, p_3, p_4, t) = 1$$

If we want to regard the choice of units of measurement as an arbitrary convention, we can replace the criterion of Section 4 by the following: *an SCPM-function is mechanically meaningful only if it is generalised Galilean invariant.*

We notice that under this new criterion the function μ_7 ceases to be mechanically meaningful. The stringent demands of the early empiricists are still not satisfied, however; for it is easy to construct a function analogous to μ_7 which does satisfy the new criterion.

(10) Let μ_{10} be the SCPM-function of type $\langle 2, 0\rangle$ such that, if $\Gamma = \langle P, T, m, s, f\rangle$ is any member of $\mathfrak{S}$, and if p_1 and p_2 are in P, then

$$\mu_{10}(\Gamma, p_1, p_2,) = 1, \text{ if } \frac{m(p_1)}{m(p_2)} \text{ is rational,}$$

$$\mu_{10}(\Gamma, p_1, p_2) = 0, \text{ if } \frac{m(p_1)}{m(p_2)} \text{ is irrational.}$$

In conclusion we should like to state two questions which arise in connection with the above analysis:

First, can the above criterion be strengthened so as to make μ_{10} and similar functions mechanically meaningless?

Secondly, is it possible to axiomatise mechanics in such a way that every SCPM-function definable in terms of the primitives is generalised Galilean invariant?

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UTILITY THEORIES IN FIELD PHYSICS AND MATHEMATICAL ECONOMICS (II) *

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A NEW and more explicit approach to Field Theory was commenced by Francis Ysidro Edgeworth in 1881. His *Mathematical Psychics* was a conscious attempt to formulate the Field Analogy with respect to Utility Theory. 'The conception of a man as a pleasure-machine may justify and facilitate the employment of mechanical terms and mathematical reasoning in social science.'[1] Edgeworth was attracted by the problem of many bodies in dynamics and by the calculus of variations.[2] (Walras' mathematical orientation was towards sectors of the classical calculus.) Edgeworth admired Lagrange, Hamilton, and Maxwell[3] who, with the aid of their 'generalised co-ordinates' solved the great problems of maxima. He recognised a magnificent analogy to the human tendency to 'accumulate pleasure under existing conditions'. He devised his model of indifference maps on the pattern of fields of forces and established the fundamental analogy for conditions of equilibrium. 'The direction in which *X* will prefer to move, the line of force or line of preference, as it may be termed, is perpendicular to the line of indifference.'[4] (The mathematical and geometrical parallelisms inherent in this statement were not formalised by Edgeworth.) The most significant advance, consciously drawn by Edgeworth from Field Theory, was his formulation of Utility as a *function with several independent variables.* Utility depends upon the combination of commodities in one's possession just as the potential function depends on the configurations of all co-ordinates. In 1871, ten years prior to Edgeworth, Stanley Jevons[5] already formulated economics as the 'calculus of pleasure and pain'. The 'Hedonimetry' of Edgeworth opened a more accomplished phase in the progress of the ' pleasure calculus '. In addition to simplified hypotheses already evolved by Jevons, the new technique of indifference maps led to the solution of more complex problems of economic behaviour. However, Edgeworth still saw no clear way of transcending the boundaries of ' hedonimetric ' abstractions and deducing a general market-equilibrium theory from his indifference maps. The most important conclusions which were to be drawn from Edgeworth's Field-analogue theory awaited Vilfredo Pareto.

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[1] Edgeworth, *Mathematical Psychics*, reprinted in the series of Reprints of scarce tracts in Economics and Political Science, London School of Economics and Political Science, 1932, 15

[2] ibid., pp. 10, 91-93 [3] ibid., pp. 11-13 [4] ibid., p. 22

[5] S. W. Jevons, *The Theory of Political Economy*, London, 1871

A significant advance in the development of the doctrine preceded Pareto, i.e. Irving Fisher's doctorial thesis in 1892.[1] As a student of Gibbs at Yale, Fisher aimed to corroborate economic equilibrium theory through vector analysis. Establishing a table of analogies, he assimilated marginal utility to force, utility to energy, disutility to work.[2] Building on the similarity between market-equilibrium and mechanical equilibria, he sought an *analogy-machine*[3] which could visualise the equilibrium of the market. He even sketched a hydrostatical apparatus in order to illustrate statical equilibrium, distribution of stocks and flows, levels of marginal utilities, etc. In our time hydrostatics can be felt to furnish only poor models for economic equilibrium theory. It is notable, however, that after a lapse of sixty years the idea of an economic analogy machine, eventually in the form of an analogue-computer based on electronics,[4] is in the foreground once again. Hydrostatics and hydrodynamics actually furnished the first working models in the theories of heat, magnetism, and electricity. Also the vision of the classical economists, including Ricardo and Mill, was unconsciously influenced by the laws of fluids and communicating vessels. Fisher's attempt was justified in his time.

The essence of Field-Theory in economics was translated in its most accomplished form by Vilfredo Pareto. Owing to his extensive background in Theoretical Physics, Pareto was more eager to develop

[1] I. Fisher, *Mathematical Investigations in the Theory of Value and Prices*, New-Haven, 1892

[2] ibid., pp. 85-86

[3] ibid., pp. 24-54

[4] A. Pikler, 'The Quanta-Kinetic model of the Monetary Theory', *Metroeconomica*, 1951, **3** ; 'Three additional Appendices to the Quanta-Kinetic model', *Metroeconomica*, 1954, **6**. (The second Appendix describes an electro-analogue simulator and a digital computer termed MONIAC, Monetary Integrator, Analyser and Computer, destined to solve steady state problems of the monetary circulation.) H. A. Simon, *On the Application of Servomechanism Theory in the Study of Production Control*, Chicago, 1952. R. H. Strotz, J. C. McAnulty, and J. B. Naines, Jr., ' Goodwin's Non-linear Theory of Business Cycles ; an Electro-analog Solution ', 1953, **21**.

the analogy than was Walras ; he revitalised the analogy, but proved equally circumspect in its formulation. Field-Analogy as such was even kept concealed and never substantiated by Pareto. In the introductory chapters of his *Cours d'Économie Politique* (1896–97) and *Manuel d'Économie Politique* (1906),[1] Pareto's great respect for methods and abstract models in Physics, especially in rational mechanics, became manifest. Studying Pareto's equilibrium theory, the initiated reader senses the very atmosphere of *Mécanique rationelle.* He recognises that certain universal principles of dynamics were copied by Pareto and applied in various economic hypotheses. The analogy, while never specified, is ascertained in its abundant manifestations.

Pareto's final objective consisted of building a positivistic substructure for use in sociology.[2] He thought of an abstract model of ' economic man ' to render intelligible all rational human behaviour. Hidden references in the *Cours* and *Manuel* betray Pareto's use of the analogy. He quoted the Principle of Virtual Displacements and stated its universal applicability. Equilibrium is a configuration of co-ordinates in which virtual variations in a system are obstructed by external constraints. D'Alembert's principle is universally applicable regardless of the meaning one attributes to the variables.[3] Every individual strives to move according to his *desires* (' *goûts* ') and to change existing configurations in the direction of these desires. However, every individual in his virtual moves is subject to certain external *constraints* (' obstacles ') imposed on him by society. Actually he will be able to satisfy his desires along certain *open paths* (' *sentiers* ') only, and when he reaches a certain *terminal point* (' *point terminal* '), namely that of maximum attainable satisfaction, he does not wish to move further. The individual's behaviour is then in *equilibrium.*

Such a universal theory of logical behaviour applies primarily to the utility-guided behaviour of ' economic man '. First, Pareto removed the mechanical analogy from its original sphere (general economic equilibrium) to that of individual behaviour ; then after having recast the theory of individual behaviour, he retransferred Field-Theory to the general sphere of market-economics. The present survey is restricted to the Paretian Field-Theory of individual behaviour.

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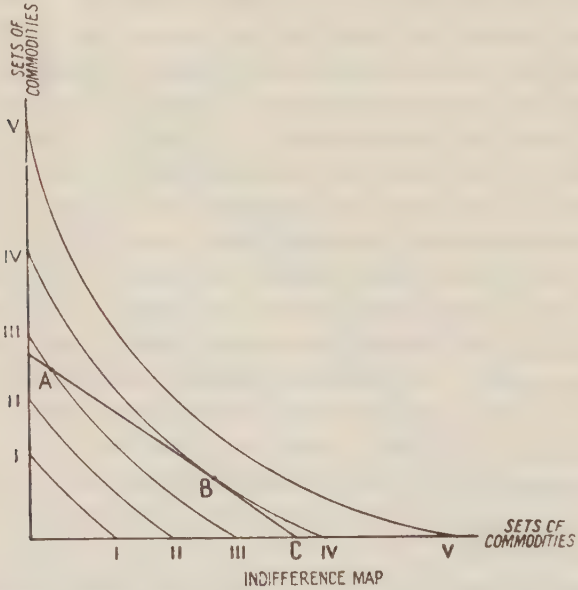
[1] Pareto, ' Anwendungen der Mathematik auf die Nationalökonomie ', *Encyklopädie der mathematischen Wissenschaften*, 1900–4, I, 1119, and *Manuel*, p. 659

[2] Pareto, *Traité de sociologie générale*, 1917–19

[3] Pareto, ' Anwendungen ', p. 1110 and *Manuel*, p. 160

Pareto based his theory of 'Ophelimity' on Edgeworth's innovation, i.e. the indifference maps. (To eliminate confusion, Pareto coined the expression 'Ophelimity' as a substitute for the conventional term 'Utility'.) He recognised that indifference-maps can be handled without assuming Utility to be measurable in a numerical metric-system. The existence of indifference-curves can be validated through immediate experience. Further considerations confirm still further the irrelevance of numerical calibration of Utility. Indifference-curves can be actually interpreted as *level-curves of the Ophelimity-function*, projected from hyperspace to hyperplane (Fig. I). A 'Hill of Ophelimity' ('*colline du plaisir*') in hyperspace may exist; however, a geometrically complete description of such an edifice in hyperspace cannot, and fortunately need not, be specified. An entire family of monotonically increasing functions, which projects identical



Point *A* represents an arbitrary allocation
Point *B* represents the optimum allocation
The index numbers I, II, III, IV, V, etc. represent 'levels' of 'ophelimity'
Line *ABC* is the income line, a tangent to indifference curve No. IV in point *B*.
The progress from level III to level IV represents gain of 'ophelimity'[1]

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[1] Corrigendum to Part I of this paper: In the legend under the figure on p. 52, the second last sentence should be replaced by the following:—The gradient of the potential function, in the plane of the figure, is normal to the level-curve at the point *B*.

contour-maps to the hyperplane, exists. To specify Utility, any of these functions would be appropriate. Such a state of indeterminacy does not conflict with the requirements of phenomenological knowledge. For explaining economic behaviour (expressed in terms of paths and extreme values) only the *order of contour-levels* is of significance ; no metric information regarding the ordinates (' Ophelimities ') is required. To be able to calculate configurations of ' abscissae ' (representing ' baskets ' of commodities) fully covers requirements of phenomenal knowledge. Henceforth, a science of economic behaviour can be firmly based on empirical constituents, i.e. *contour-maps of the Ophelimity-function.*

The Figure represents an indifference-map pertaining to the consumer's behaviour. The indifference curves I, II, III, IV, etc. represent loci of *equally useful ' baskets '* of commodities. The indices I, II, III, IV, etc. denote the *order of utility levels.* Line *ABC* is an *income-line*, i.e. a locus of baskets of commodities attainable on a given income. Since price-level is an invariant parameter for an individual, income-limitation designates a *linear* type of constraint. Equilibrium of consumer behaviour materialises at the point of tangency *B*. This point designates *optimum allocation* of income.

. . . . .

The following table comprehends a concise survey of the mathematical and geometrical key-concepts of the Paretian Ophelimity Field-Analogy. This analogy is drawn between the dynamics of a single particle placed in a field of force, and an individual's economic behaviour in the ' field ' of his own desires. It is understood that the particle moves only in the continuum of a three-dimensional manifold (space) ; the individual moves on the *n*-dimensional surface of the commodity-manifold. The correspondences are as follows :

Field	*Commodity-Hyperspace*
Co-ordinates	Co-ordinates
Abscissae (specified as ' triplets ' to describe the particle's location in space)	Abscissae (Specified as *n*-fold ' bundles ' to describe commodity-baskets)
Hyperplane (manifold)	Commodity-plane (commodity-manifold)
Ordinates (representing the particular values of the potential)	Ordinates (representing the particular values of Ophelimity)
The four-dimensional configuration (' hill ') of the potential	The $(n + 1)$-dimensional configuration of ' hill ' of Ophelimity

Field	*Commodity-Hyperspace*
Lamellar character of the Field	Lamellar structure of the 'hill' of Ophelimity affording the projection of a perfectly ordered contour-map to the commodity-plane
Potential surfaces	Indifference-surfaces
Potential curves	Indifference map in the projection
Parameters (constant factors outside the Field)	Economic parameters (non-interacting, constant implications in the environment)
Surface of constraints	Surface of income-constraint
Scalar quantities	Economic scalars (e.g. commodity-stocks) additive according to algebraic rules
Vectors	Economic vectors, i.e. quantities additive in terms of an 'operator' (Example: scalar values of the marginal utility-components)
Gradient of the potential (force vector)	Gradient of the Ophelimity function (marginal utility as a vector)
Magnitude of the force	Marginal utility of the total basket (identified often as the marginal utility of 'money')
Direction of the force	Preference direction (corresponding to the normal direction, i.e. maximum rate of change)
Lines of force	Preference lines
System of lines of force	Preference map
Rectangular components of the gradients (the 'scalar factors')	Marginal utilities of the single commodities considered separately (partial derivatives of the Ophelimity function)
Unit vectors	A set of multipliers derivable from the Ophelimity function
'Operator'	'Operator'
Path element	Path element
Finite path	Finite path
Decrease of potential	Gain in Ophelimity
Irreversible path	Irreversible behaviour
Increase of potential	Loss in Ophelimity
Reversible path	Reversible behaviour
Locus of a particle's static equilibrium	Locus of optimum income-allocation on the commodity-plane

The Paretian Field-Analogy functions as a didactical model to explain complicated concepts and also to illustrate various hypotheses about the behaviour of systems. Even in the light of modern physics, this Analogy proves flexible for further specifications. The Analogy functions conversely also, namely, in the anthropo-morphic way: nature behaves *as if* she would follow rational considerations of economic strategy. The following are the main derivations:

(1) *Metric considerations.* Uniquely valid metric-systems are not postulated for the specification of a Field-function in either Field-Theory. Just as a calibration of Utility in a numerical scale is irrelevant, so a uniquely valid metric scale measuring potential, is not postulated. *Different metric-systems, if monotonically covariant, are equally justified.* (Pareto was a forerunner of those who found metric considerations inherent in the theory of entropy, in the general theory of relativity, and also in certain metric aspects of recent developments in mathematical psychology, the theory of games, and information theory.)

(2) *The generalised concept of Field-functions*; *variability dependent on allocation.* The potential of a dynamical system depends upon the allocation of its particles; potential can be created and annihilated through changes in allocation. Similarly, total ophelimity of income depends on its allocation. Ophelimity can be created and annihilated, depending on allocations.

(3) *Determinacy of Field-behaviour*; *properties of a Field-function.* Determinacy of Field-behaviour is conceivable only if the field-function is invested with certain mathematical and geometrical properties (its lamellar character, etc.) specified in vector analysis. A further assumption is the existence of constraints imposed on the variables. Constraints can be linear or non-linear.

In Ophelimity Theory, determination of the behaviour of an individual is based on similar mathematical criteria. An ordered contour-map encompassing a *system of non-intersecting level-curves*, is postulated. Income-limitation associated with a given price level designates the constraint. At a constant price-level the constraint is linear.

(4) *Extreme values.* The ordered lay-out of a potential field explains the existence of minima under specified constraints. A similar order prevailing in commodity-hyperspace secures maximisation of Ophelimity (optimum allocation of income).

(5) *Geometrical formulation of the condition of equilibrium.* In

equilibrium, the virtual displacements of the particle are constrained to the minimum attainable level of the potential; at this point, the surface of constraint is perpendicular to the gradient.

In the state of optimum income-allocation, the consumer's virtual moves are constrained to the highest attainable indifference level. At this point, the Ophelimity-gradient is perpendicular to the surface of income-constraint.

(6) *Points and regions of maxima and minima.* In the appropriate part of statics, equilibrium is specified as a single point. When dynamics is involved, the motion of the particle will extend over a surrounding, bounded region.

Similarly, in the static theory of optimum-budget, equilibrium is specified as a point of the commodity-plane. In the dynamical situation, optimum-behaviour is associated with small oscillations around point-equilibrium.

(7) *Superimposed constraints*; *equilibrium under stress.* Added constraints will not permit the original value to materialise; they will allow only a relative minimum compatible with the new boundaries of motion.

Relative optimum is a familiar concept in Utility Theory. When government imposes constraints on purchases (e.g. in the form of rationing), only a confined optimum on the commodity-plane is accessible.

(8) *Multiple equilibria*; *metastability.* The structure of a potential field may allow a multiplicity of extreme values in non-contiguous regions. Accordingly, multiple equilibria are actually possible in statics (e.g. a system can have several equilibrium positions: stable, unstable, or metastable).

The feasibility of multiple equilibria, as a possible outgrowth of the particular properties of the Ophelimity Function, is a familiar issue in utility theories also.

(9) *Irreversibility.* Nature's spontaneous and irreversible tendency is to decrease potential. For her the potential function expresses levels of *disutility* in particle-allocations. Similarly, the economic behaviour striving to increase Ophelimity by means of allocation is spontaneous and irreversible.

(10) *Reversibility.* In the dynamics of a particle, all motions performed around the point of static equilibrium are reversible; thus any given event-history may repeat itself in the reverse order. Moreover, reversible processes can be engineered by imposing a stress on the

system. After the stress is released, the system will complete its reversible cycle and return to its initial equilibrium position.

The consumer's behaviour in his dynamic equilibrium encompasses reversible motions performed on the commodity-plane. Moreover, reversible cycles can be artificially generated. For example, when the government introduces constraints on free budgeting to be gradually released afterwards, the consumer will perform a reversible cycle on the commodity-plane.

(11) *Stress of the system in the reversible process. Le Chatelier's generalised principle.* The system, under the stress of superimposed constraints, behaves as it would *resist* any added constraints by the outside agency. (Such particular interaction of system-co-ordinates can be stated not only in thermodynamics, but in sections of classical mechanics also.)

In the dynamical theory of the budget, a corresponding phenomenon of ' stress ' is well known. The consumer's behaviour always *opposes* the constraining agency. (Examples: The government levying a new sales-tax; a monopolist increasing the price of his product. The consumer's reaction to these constraints will consist in reducing the volumes of his taxable transactions and of his respective purchases.)

(12) *The principle of ' independence of the paths '.* The future of a particle depends on the point it actually occupies in the field; the preceding history of paths is irrelevant for the determination of the future (as far as statical equilibrium is concerned). A similar indifference characterises the consumer's behaviour also.

(13) *Quantal indeterminacy.* According to Heisenberg's principle, the elementary paths are subject to indeterminacy. Similar indeterminacy at the quantal level (a quantal-indeterminacy of allocations), can be traced in Utility Theory also.

(14) *The dynamical theory of Field. Field transformations.* Interference by an external agency with an existing field of force will result in changing the structure of the Field. Due to intervention of one common operator, the *ensemble* of all potential surfaces will suffer corresponding curvatures. Intrinsic structural changes may transform the original constraints. Modern field-physics studies substantially such transformations (' warpings ') of the continuum.

The conception of Field in Ophelimity Theory is also not rigid. Due to changes in desires, a utility contour-map can suffer significant transformations; the common operator, ' change of desires ', will alter its curvature on the existing contour-curves. Furthermore, due to a change in the income, or in the price-system, the specification of the income-surface will also change. Adequate shifts in economic behaviour will result. A dynamical theory of the consumer's behaviour in terms of changes in the continuum, seems to be the appropriate mathematical approach.

(15) *Interactions of Fields.* The laws governing the interactions of Fields constitute the fundamental problem in modern physics. A unitary science of all types of interacting fields would imply the highest attainable level of human knowledge, as was said long ago by Laplace, and, in our day, voiced by Einstein.

Pareto attempted to formulate a general equilibrium theory in terms of the behaviour of interacting individuals. Complex economic equilibria result from the coupling of single equilibria ; by coupling simple systems complex systems can be understood. Pareto's approach to general equilibrium corresponds to current approaches in field-physics. However, it is beyond the scope of this study to delve any deeper into Pareto's thoughts.

The hidden Paretian Analogy was never substantiated in economic literature following Pareto. With few exceptions (Osorio,[1] Moret,[2] Moore,[3] Divisia,[4] Amoroso,[5] Rueff,[6] Davis,[7] Samuelson,[8] Lisman[9]), the general trend of mathematical economics became antagonistic to so-called 'analogies'. Most strikingly, such turning can be elucidated in its parallelism to the history of mechanics.

In mathematical economics Walras and Pareto represented the rôle of Newton and Lagrange ; they were the '*cosmologists*' in economic theory, searchers of *complex system-behaviour.* A third orientation in mechanics, more modest, yet more realistic in its scope, also exists ; it is the 'kinematic' approach founded by Galileo. Kinematics suffices with a concrete description of isolated systems without examining generating forces. Experimental science of our age, tending to study carefully isolated processes, is actually anchored in Galileo's physics. The post-Paretian positivistic trend in mathematical economics seems to have favoured Galileo's sober realism. To be noted is the fact that Alfred Marshall's[1] market theory was directed towards the study of single, isolated markets, i.e. *partial economic equilibria.* In a similar system, Field Analogy, which was indispensable for the conception of general economic equilibrium, lost ground. No mechanical model is needed to make equilibrium of single markets understood. Finally the contemporary American trend in econometrics was overwhelmingly influenced by advances in the techniques of mathematical statistics. Field-physics is not compatible with a similar approach either.

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[1] A. Osorio, *Théorie mathématique de l'échange, Paris*, 1913

[2] J. Moret, *L'emploi des mathématiques en économie politique*, Paris, 1915

[3] H. L. Moore, *Synthetic Economics*, New York, 1929

[4] F. Divisia, *L'indice monétaire et la théorie de la monnaie*, Paris, 1926, and *Economique rationelle*, Paris, 1928

[5] I. Amoroso, *La cinematica economica in un mercato chiuso*, Roma, 1924

[6] J. Rueff, *Théorie des phénomènes monetaires*, Paris, 1927

[7] H. T. Davis, *The Theory of Econometrics*, Bloomington (Ind.), 1941

[8] P. A. Samuelson, *Foundations of Economic Analysis*, Cambridge (Mass.), 1947

[9] J. H. C. Lisman, *Econometrics, Statistics and Thermodynamics*, The Hague, 1949

Opposed to all above orientations, Keynes's[2] theory of economics denotes a return to the abstract concept of general economic equilibrium. Keynesian models could actually be formulated along the lines of mechanics (although Keynes was apprehensive of explicit use of ' analogies '). The modern concept of ' servo-mechanism ' based on ' information ' and automatic control through '*feedback* ', seems inherent in Keynesian theories concerning progressive economic trends. To pursue this line of mechanistic thoughts is also beyond the scope of this study.

Did the science of mathematical economics properly exhaust instructive and heuristic methods and models in field physics ?

The writer of this study suggests a macro-economic model of the market based on the reformulated Walrasian Collective-Satisfaction Function. To describe the *ensemble* of flows of all commodities and services flowing to and from the market, the *macro-economic continuum* has first to be specified in the form of an ordered vector field. The manifold of this continuum, the *macro-economic manifold,* will encompass the total variational area of the ' *generalised* ' *macro-economic co-ordinates* (the latter denoting flows of commodities and services). The additional co-ordinate to be drawn perpendicularly above the manifold-plane is Collective Satisfaction. In terms of measure theory it is readily understood that this function is built on *scalar* values, measurable merely in the ordinal, *index-scale.* As generally in vector-analysis, it is not feasible to derive the ' existence ' of a field-function on a clear-cut empirical basis. The assumption of existence has to be tempted and tested in its various applications. The field-physicist's criterion is *order*. The permanent order of daily flows in steady-state economics (to which we are naïvely accustomed, but which, nevertheless, did not fail to evoke Ricardo's surprise and admiration),[1] suggests *field determinacy* in the field physicist's mind.

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[1] Marshall, *Principles of Economics,* 1890

[2] J. M. Keynes, *The general theory of employment, interest and money,* London, 1936. For the *feedback* interpretation see Arnold Tustin, ' Feedback ', *Scientific American,* Sept. 1952. For the ' cybernatic ' effect see A. Pikler, *op. cit.*

A field model of macro-economics could be developed as follows:

(1) The characteristic function (Collective Satisfaction) is single-valued and monotonically increasing with each of the independent variables.

(2) The characteristic function projects a completely ordered contour-map on the manifold-plane, i.e. the *social contour-map of preferences and indifferences.*

(3) With regard to any chosen point on the manifold-plane, scalar factors, operators, gradients, and their vector-components can be determined.

(4) The actual variation of the macro-economic co-ordinates is subject to *constraints*. In the light of vector-analysis, social constraints are translated into terms of *constraining surfaces*. To each particular type of economic system (free competition, monopolies, state interventions, co-operative systems, etc.), a characteristic constraining surface can be associated.

(5) From the instantaneous configurations of contour-maps and maps of constraints, static equilibrium (steady state) can be derived. Geometrical implications (i.e. the issues of tangency), and the corresponding matrix-properties of the resolving equations will actually account for *finite* and *bounded equilibrium-areas*. Thus the essentially oscillating nature of equilibrium appears to be an inherent property of the vector-field, rather than simply a random-sequence of resolution-errors in the Walrasian sense. Also the possibility of *multiple-equilibria* (metastability) is inherent.

(6) Regarding the detailed history of equilibrium-*oscillations*, the static model remains non-informative. To specify time-flow, some generalisation of the economic vector-algebra, possibly on the pattern of Hamilton's, is needed. A unified space-time continuum can be introduced. Each bundle of vector-operators could be supplemented with an additional set of vector-operators ('versors'), the latter accounting for a characteristic *time-lag*. (The complete bundle of $n + 1$ vectors will constitute a 'multernion'.) The economic

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[1] Ricardo, *Principles of Political Economics*, 1817

meaning of the said time-lags is associated with the *rates of turnover;* the reciprocals of these rates are known to represent '*average life-time*', or '*relaxation time*' (a terminology borrowed from quantum mechanics). A perfectly ordered lay-out of the said time-lags in the *imaginary domain* of the vector field will afford flexible reformulations of system-constraints. On the pattern of the 'non-holonomic' systems, all types of liaisons between co-ordinates and time-flow, can be included and treated. Provided one could trace a principle quite general in its scope, similar to Hamilton's Principle in dynamics, co-ordinate variations and time-flow could be inseparably interconnected and the actual motion of the system described in its most general ('canonical') form.

The assumption can be made that the 'dynamic path' of the economic system (opposed to all conceivable 'variational paths'), will be characterised by the tendency of the system to *maximise the time-integral of Collective Satisfaction under the existing constraints.* Correspondingly, a *canonical equation of motion* could be thought of as based on the properties of the vector field in which a particular system is actually imbedded. The dynamic economic event-line will obviously not follow the steepest ('normal'), nor the shortest pathways ('geodesics') of the continuum. The Principle of the Time-Integral will cover *harmonic* and *non-linear oscillations* in steady state economics an account also for the '*transient*' *processes* of build-up and decay (conventionally treated as progressive and regressive trends in Keynesian economics).

(7) 'Hamiltonian economics' are still confined to *isolated* (i.e. non-interacting, 'conservative') systems driven by their own internal forces. The Hamiltonian step now could be followed by the one corresponding to Maxwell's. Any exogeneous, acting force can be conceived as a *central operator* impressing corresponding *transformations* upon the vector field by '*warping*' the continuum, or by reshaping the constraints. One may be able to formulate differential equations analogous to those of Maxwell. In the present light, dynamic economics represents essentially continuous *interactions between changing contour-maps* and *changing constraints.*

The validity of Le Chatelier's Principle in economic interactions (recognised by Divisia,[1] Rueff,[2] the author,[3] and Samuelson,[4] furthermore by the thermodynamicist Klemenciewicz[1]), will also 'fit in' well into the present reference-framework. When a system is affected by an outside system, the internal co-ordinates cannot fail to respond in any other way than by *counteracting* the active co-ordinate-variations in the interfering system. The ultimate explanation of the analogy is, however, in the domain coming under the entropy principle.

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[1] Divisia, op. cit.

[2] Rueff, op. cit.

[3] A. Pikler, 'Theorie der automatischen Transferfähigkeit', *Ungarische Volkswirtschaftliche Rundschau*, 1934, No. 5, and *op. cit.*

[4] Samuelson, op. cit.

(8) The 'Einsteinian step', i.e. relativisation of economic time-flow could not be justified by the facts. Economic information is practically simultaneous and space-time liaisons (example: international arbitrage), tend to yield non-contradictory transformations.

(9) *Quantisation* of the co-ordinates would afford the transition from 'classical field-economics' toward 'quantum economics'. It can be shown that the total number of quantised macro-economic configurations is *finite* and their ordered ensemble constitutes a 'quantum Field' (obeying, in the first approximation, Einstein-Bose statistics). Degeneracy, wave-functions, entropy could serve as key-concepts toward the *quantum-kinetic theories* of *commodities* and *of money*.[2] However, the scope of our paper is confined to classical field-models.

Can field theory be considered as true? A sceptic of our time,[3] ingeniously referred to the 'smile of a remote cat'. The criticism is justified. Indeed, the entire philosophy underlying field physics reveals our world as constituting merely a theatre of shadows projected from the Absolute to the plane of observable dimensions. The theory of relativity rejects even any reference to the Absolute.

Perhaps the most appropriate response to the doubts of the sceptic is the quoting of young Irving Fisher's motto: 'It is the "old story" of the practical man versus the scientist.'[4] To understand complex systems' behaviour, by using inference to supplement the missing links of incomplete information, is perhaps the principal objective of the human mind. 'Nature book is written in the language of mathematics', said Galileo. The behaviour of complex systems appears to be recorded in nature's open book in the most abstract of all code-languages, i.e. *mathematics of multivariables.* Although the human mind will never be able to understand every coded word, it will, fortunately, be capable of grasping the universal logic of the encoded text, i.e. *Field Theory*. Three instances in Field Physics during the past century and a half may be convincing. On the basis of Lagrangean dynamics Laplace was able to calculate the orbits of planets. Field Theory of electrodynamics enabled Maxwell to predict the existence and laws of electromagnetic waves. On the basis of the relativistic theory of the Gravitational Field, Einstein could foretell the equivalence of matter and energy.

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[1] Z. Klemenciewicz, 'Sur une définition générale de l'équilibre et de la stabilité', *Scientia*, 1949, **43**, 120, 124

[2] A. Pikler, 'The Quanta-Kinetic Model of Monetary Theory', *Metroeconomica*, 1951, **3**, 70–95

[3] Herbert L. Samuel, *Essay in Physics*, Oxford, 1951

[4] Fisher, op. cit.

Regardless of the manifold sectors of phenomena in the world, whether they are physical, biological, psychological, or social, the human mind cannot renounce its most powerful tool for the understanding of complex systems, i.e. *reasoning in terms of Fields.*

MATHEMATICAL APPENDIX

To formalise the Pareto-d'Alembert-Lagrange Analogy, this Appendix will envisage only the simplest case of market-equilibrium based first on barter, then on money.[1] Implied are n persons, k types of commodities in initial quantities x_0, y_0, z_0 . . ., and an unknown price-level which can be referred to any of the commodities as a chosen standard ('*numéraire*') and specified as sets p_x, p_y, p_z . . . In the case of income expressed in money, price level is specified as one common set of p's.

A necessary condition for equilibrium to exist between Ophelimity-gradients $\psi(x)$, $\psi(y)$, $\psi(z)$ . . ., their marginal utility-components, and the final commodity-allocations x, y, z . . . is that virtual Ophelimity-gain for any arbitrary modification in any person's budgeting, compatible with the liaisons, be zero. This will materialise if Ophelimity-gradients (specified separately for each person) are proportional to prices regardless of '*numéraire*', i.e.

$$\psi(x) = \frac{1}{p_y}\,\psi(y) = \frac{1}{p_z}\,\psi(z) = \ldots \qquad (1)$$

Meanwhile the algebraic sum of receipts and expenses computed in 'numéraire' must be equal to zero; i.e.

$$(x - x_0) + p_y(y - y_0) + p_z(z - z_0) = 0. \qquad (2)$$

In the more realistic model of allocating a total income I, the constraint is

$$I = F(x, y, z \; . \; . \; . \; p). \qquad (3)$$

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[1] *Manuel,* p. 569

The chain of $(n-1)$ equations under (1) expresses *zero gain* in all virtual moves. Equations (2), (3) respectively, express *constraints* on moves. A set of Ophelimity-functions to secure solvability for all conceivable price-levels and incomes must *exist* as shown by Pareto.[1]

The mechanical analogy exists with the simplest types of mechanical systems in which specification of constraints and distribution of force-vectors are independent. A necessary condition for equilibrium to exist between forces upon the particles is that virtual work of the forces for any arbitrary displacement compatible with the liaisons be zero ; i.e.

$$x\delta x + y\delta y + z\delta z + \ldots = 0. \qquad . \qquad . \quad (1)$$

One can write as many equations as there are virtual moves admitted. An additional equation will specify the constraints in the form

$$F(x, y, z \ldots) = 0 \quad . \quad . \quad . \quad . \quad (2)$$

and constrain equilibrium correspondingly.

A basic assumption in Field Physics is that the Field-Function *must* exist.

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[1] *Manuel*, pp. 547-556

LABORATORY VERSUS FIELD RESEARCH IN PSYCHOLOGY AND THE SOCIAL SCIENCES*

VIRGINIA BLACK

IT IS not infrequently asserted by those interested in the broad sweep of human behaviour in everyday life that because laboratory investigation in psychology and the social sciences is restricted in its methods of investigation, its subject matter is accordingly limited. On the other hand, enthusiasts of laboratory investigation claim that adequate generalisations can be made about all behavioural phenomena, indeed, that theirs is the only way to make generalisations which will bear repeated scrutiny. The purpose of this paper is to examine the methodological strong points and shortcomings of both these claims, with a view to a possible resolution of the controversy.

The laboratory does seem unable to reproduce some intricate activities, like discovering mathematical formulae or composing songs, making treaties, or making war. Because of this, some social scientists [1] have contended that the laboratory cannot cope with these complicated phenomena. Control, essential to experimentation, vitiates investigation based on it; it distorts the circumstances in which these activities are found. Because it weeds out and isolates, holding some features constant, leaving others variable, it imitates reality but it is not itself reality. At most it is a reasonable facsimile. Experimental purlieus are so changed by this elimination of context, alteration in the quality of activity experienced, and artificial reproduction, that results gleaned from them are worthless. They are neither descriptive nor explanatory of extra-laboratory activities.

The argument continues: But simpler phenomena, such as, for example, primitive reflexes, need habits, rote memorisation—these the laboratory is equipped to investigate because they are reproducible in the laboratory. They are easy to set up. Compounded of fewer strands of experience, they are naturally easier to manufacture without doing violence to the distinguishing *quale* of the phenomenon being

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* Received 31. i. 54. Thanks are due to my husband, Percy Black, for assistance in giving this paper its final form.

[1] See, for example, the argument by W. A. Peterson and N. P. Gist, 'Rumor and public opinion', *Amer. J. Sociol.*, 1951, 57, 159–167.

considered. When, however, the laboratory turns its attention to higher-order phenomena, including all group activities, it goes beyond what its method is capable of handling. It is the field sciences, investigating directly situations-in-life, that succeed in formulating generalisations applicable to life. This is because they do not need to duplicate what they study, and hence do not run the risk of creating an experimental situation which has little in common with life.

The foregoing rejoinder to laboratory enthusiasts does not assume that there are limitations inherent in psychology or sociology, because, in theory at least, everything that happens, including therefore all human behaviour, is amenable to the sort of investigation which yields correlations between sequences of events believed to be dependent on other sequences of events. For example, to cite an instance of a psychological problem, hatred of religion might be shown to be correlated with a protracted submission in childhood to rigorous religious discipline. Indeed the scientific method postulates illimitability in scope of subject.

Rather is the question one of tactics : how can we best explain the more encompassing and complex individual and group activities ? If the laboratory is restricted by its need to control, manufacture, and attenuate, then perhaps we must not expect it to contribute to the body of law applicable to social phenomena. Can any experiment, for example, bring to light factors endemic to religious retraction or prejudice ? Or must we go to the field for explanations of long-range attitudes, many-compounded social processes and human pursuits ?

Experimentalists in the psychological and social sciences usually recognise that they cannot reconstruct in detail social phenomena which are complex or prolonged, such as, say, trade relationships between adjacent primitive cultures. A culture cannot be grown in the laboratory ; it is necessary to focus investigation on the culture itself, as and where it exists. If the mountain will not come to Mohammed, Mohammed must go to the mountain. Likewise, composite occurrences such as mass hysteria, the growth of rumour or sibling rivalry, creative intuition, cannot be authentically laboratory-bred so that all their components can be tallied, measured, or foreseen. Within the compass of four walls, we cannot nurture from birth to death a pair of twins, or catch a poet spontaneously at work, or mirror a crowd uprising. Rather must we arrive at laws about these elusive and mosaic-patterned activities from our observations in the field, that is, from actual existing *unselfconscious* representative cases.

LABORATORY VERSUS FIELD RESEARCH

The controversy between the methodology recommending field survey and that upholding experimental research in the laboratory hinges on two contingent questions : (1) What in each case is the type of problem being investigated ? (2) What is the distinction between laboratory work and field work ? These questions will be treated separately below.

I

Studies dealing with phenomena which are literally too big to be reproduced in a control room must be examined wherever and whenever they occur—that much is true. Field investigators who decry experimentation, however, seldom limit their objections to activities which are too clumsy to fit the spatial confines of a laboratory or too nearly unique ever to be repeatable. The following argument, for example, classifies all group phenomena as being impossible of experimental success.

> The major limitation in the experimental study of rumor, and of other forms of collective behavior lies in the failure to produce, or even to simulate, affectively toned motivational states comparable to those which occur in real life. This is true both of the small discussion group and of the public.[1]

Elsewhere in that article, its authors suggest that affect and motivation are also laboratory-exempt because something in their nature, something intrinsic in their make-up, prohibits reproduction without artificiality, an artificiality which renders any generalisations formed from the reproduction inapplicable to life. The counter to this argument rests partially on what is meant by comparable state, and clarifying how we know when we have one.

If a state is simple, like an eye-blink, its similarity to other eye-blinks is immediately discernible. If a state is complex, like a political revolution, it probably possesses no equivalent. But if we can perceive or measure similarities of structure (two projective drawings), organisation (two religions), number (equal I.Q.'s), sometimes of concurrence (two neuroses) or location (two outbreaks of delinquency), and soon, then we may say we have a comparable state.[2]

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[1] Peterson and Gist, op. cit.

[2] Compare with problems in the philosophy of science concerning identity of response, event, act, and so on.

Classifying events and things requires a certain amount of arbitrariness. How many qualities, or which of them, we decide to include in the definition of something or to correlate with its behaviour depends to some extent on language conventions, and upon practical convenience in manipulation. Trial-and-error experimentation usually discloses the solution. Frequently we redefine and reorganise our language to fit new situations which arise both in science and in everyday life. This practical motivation lies behind many of the shifts we make in thought and speech.

Analogously, what we decide to regard as a *thing* (*event* or *situation* or *state*) is also arbitrary. If the aim of science is to connect conceptually all happenings, then we must know how many, or how much, or exactly what constitutes a happening. In specific instances this depends on the inclination of the investigator. He may confine his researches to visceral changes (contractions, distensions, and so on) in the stomach walls during extended starvation. Or he may include in his researches the manifold variety of social ebb and flow during a depression or a boom. How broadly he flexes the subject matter of his study—how broadly he interprets its confines—is a matter of his own choosing. And it is this choice which initially determines for him what he will regard as a state.

Once determined, observation and experiment reveal the similarity of his defined state to other states. His generalisations, or laws, are the fruitful consequence of his investigation.

Scientists who oppose laboratory study in the areas mentioned deny that the laboratory can create activities which have sufficient in common with extra-laboratory activities, like being prejudiced or having the urge to commit a crime or deciding to co-operate with one's fellows, to warrant investigation which ensues in generalisations legitimately applicable to life. They insist that tailor-made activities do not fit. Reality is ill clothed if it must rely on the laboratory to predict its recurrences.

But advertising, personnel selection, psychological warfare—all these applied fields borrow regularly from the findings of laboratory scientists, showing that laboratory generalisations are indeed applicable to life.[1] For example, the emotive effects of yellow and the persuasive effects of repetition are well known to propagandists, whether it be shoes or vegetarianism they are trying to sell. The testing of the hypotheses describing both these tendencies involved the creation of affect in the laboratory.

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[1] See, for example, Gordon Eckstrand and A. R. Gilliland, 'The psychogalvanometric method for measuring the effectiveness of advertising', *J. Appl. Psychol.*, 1948, **32**, 415-425; or L. W. Doob, *Propaganda*, New York, 1935.

Asch's power experiments[1] illustrate the success of the laboratory in inducing affect. In comparing the length of vertical lines placed serially, naïve subjects were observed to yield to or resist the influence of their instructed peers in publically announcing their estimates. Being different from others, perceiving inaccurately, or being confronted with the conflict between majority opinion and minority dissension induced stress and anxiety which were clearly discernible in these experiments. The results shed light on the dynamics of personality in one of its rudimentary postures—independence of judgment when under group pressures. The pattern of human behaviour under social duress revealed by these experiments seems no different from its frequent counterpart in everyday life.

Perhaps the surest answer to be offered to scientists who are sceptical of the experimental procedure is the practical dictum : try it and see. Whereas success in predicting usually means success in method, failure in predicting does not always mean failure in method.[2] The method must prove itself just as a law must prove itself—by successful prediction.

But the theoretical problem remains. Can we shrink life to the dimensions of the laboratory ? Are laboratory replicas of reality convertible ? Can comparisons between them be made ? If so, can we predict what life will unfold, even if we have not observed it ?

Laws derived from experiments are always discrepant with the same laws deduced from formulae worked out *a priori*. In the physical sciences, for example, constructs such as *vacuum*, *point*, *atom*, *wave*, have no referents in ordinary experience, and the ideal curves formulated in these sciences seldom parallel the curves obtained from the plotting of data. Similarly, psychology finds it useful to posit *average man*, *trace*, *drive*, *normal curve*, none of which concepts have ostensive referents corresponding to them or their derivative hypotheses.

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[1] Solomon E. Asch, *Social Psychology*, New York, 1952, pp. 450–501

[2] Whenever scientists claim that hypotheses are true because of correct prediction, they are in effect committing the logical fallacy of asserting the consequent. For this reason it is possible to predict accurately with false assumptions, or even to predict unsuccessfully with true assumptions when the initial conditions are misinterpreted. Since there is no infallible method to rule out error in prediction, findings are regarded only as probable, never as certain.

The analogy to be drawn is that the laboratory, in isolating its variables, produces generalisations which are like these theoretical constructs in their usefulness. They are devices for rationalising our experience, and are useful just because they are *not* descriptive or explanatory at the level of direct observation. No instance of application is supposed for some of the higher-order laws in the physical sciences, yet precisely because they are more general and less concrete than generalised observation-statements on the ' All cats have tails ' level, they embrace and subsume more phenomena. Similarly, if we suppose no counterpart of our experimental caricature to exist in life, we will not be grieved at not finding it. We will instead show how the procedure of the laboratory can highlight and inform natural processes, thus providing us with a skeleton which enables us to predict events in the same way that a magnifying glass opens up an unexperienced and otherwise undiscoverable world.

Where the social laboratory thus fails to duplicate life, its findings are not *ipso facto* of no consequence. By its very selection and discrimination, it so reshapes reality as experienced that the effervescence of actions is momentarily stilled. The solitude of the laboratory is like an empty room in which a pin is dropped. Its resound could not be heard at all if the room were filled with things. ' Distortion ' signifies the experimentalist's unwillingness and inability to investigate more of a topic than he can focus on with the apparatus or method at his disposal. Since he cannot bring rumour into his study and reproduce there everything included in it, such as geographic spread, need gratification, duration, ellipsis, he must investigate at a time only certain strands of rumour events ; but his results are inordinately more general and exact because he has kept his study proportionately intact. The best example of contortion facilitating explanation, and subsequently prediction, occurs in pathology where the deliberate perusal —the deliberate creation—of physical aberrations furnishes a means of understanding normal development. Freud argued that the exaggerated effects of abnormal behaviour revealed the workings of the human mind far more poignantly than those activities under ordinary circumstances. Here life perverts itself, exhibiting ready-made examples of activities whose characteristic features protrude with the same sharp edges magnified in the laboratory.

Control has its virtues as well as its vices. The events dealt with may be fewer than the field can handle, but the ability to duplicate, not contextual life situations but particular threads of events shared by several or all contexts of a given *quale* (for instance rumour), suggests that laboratory findings reveal the regularities that lie beneath the felt and the seen far more successfully than might appear. They do not describe reality at the observation level, but they grapple with it nearer its source, just as physics demonstrates a sun that we never see with our eyes on a fair day, but a sun far more influential than perception could ever surmise.

This leads us to consider similarity and uniqueness. In defining *situation* or *comparable state*, we saw that arbitrary decision plays a rôle in determining which phenomena shall constitute a given study, the choice depending upon convenience and upon the skill of the experimenter. Our sense-organ equipment is so constructed that we readily see some things and not others. Some relationships seem to us to stand out and fairly shriek their likeness to other relationships, hence all class-and-kinds decisions are not equally arbitrary. Pictures and poems, for instance, seem to us more or less obvious choices in grouping things. We perceive quite easily that two identical pictures were drawn by the same psychotic, or that two written manuscripts are both the effort of a single genius. We call these durable entities ‘ things ’. But taking a level of aspiration test, or developing a compulsion neurosis are phenomena less easy to classify. We call these fleeting entities ‘ events ’, and have no ready names for them. Their character is heterogeneous, their properties are evanescent. Thus we are hard put to label or correlate them at all. Often they are one of a kind. In what we should regard as their totality they seem never to be duplicated or repeated.

But in fact they do have generic features. Compulsions, for example, display similar syndromes ; we can assemble for a large number of cases symptoms which bare striking resemblances to one another. They must possess some likenesses ; otherwise they should not all be compulsions. Such differences as they show, we assume to be a product of their different contexts—for example, their occurrence in different persons, or at different times, or in different environments—in short, their aetiologies.[1]

The difficulty of generalising and predicting in the psychological and social sciences results in large part from this human inability to perceive the large-scale and long-scale dimensions in man and men. The technical apparatus of the laboratory is designed to free the experimenter from his everyday spectacles, and to substitute for the raw vision of commonsense the refined binoculars of instrument and control. Far from enabling more adequate predictions, field research actually can obstruct its predictive capacity by the *sui generis* amorphousness of the activities it is often wont to study. The sheer impossibility of observing social itinerants like crowd panic, culture infiltration, unconscious motivation, in-group pressures, prejudice and so on, impedes law-seeking until these activities are experimentally dismembered. So multiform and time-bound are these social phenomena that none of them ever reproduces itself *in toto*. Hence except for some commonplace assertions which could almost be regarded as tautologies, like ' Culture infiltration requires accessible trade routes ', generalisation is impossible. In their totality, the uniqueness of mass events disqualifies them from being subsumed under laws, for by definition a law describes a relationship holding between all or a given number of situations of a given kind. Only when the data of a piece of research are delimited to the extent that common features are recognisable is science possible. But if we cannot construct in the laboratory situations identical in kind with those outside the laboratory, then certainly the macroscopic reality which constitutes the data for the psychological and social field sciences cannot reproduce itself *even in life* sufficiently to warrant more than awkward guesses as to what future events will be like. Any man can prognosticate with some degree of success. Science must do better than commonsense to justify itself.

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[1] Analogously, hallucinatory symbols derive from one’s culture. Back-area peoples are not pursued up elevators by imagined enemies. It is doubtful whether western man experiences head-hunting hallucinations, although enlightenment gives him the advantage over his uncivilised neighbour with respect to symbolic variety.

Predictions must be hazarded on the basis of both kinds of studies, and tested. The proof of the method and the confirmation of the hypothesis are equally at stake in this pragmatic test of truth scientifically pursued.

2

In the preceding discussion, we examined the limitations of laboratory experimentation resulting from the inability to bring indoors certain social phenomena. Critics of controlled research hold also that affect, intergroup reactions, motivation, and generally some of the more intricate learning processes involving more than brief pen-and-paper tasks like memory work, recall, retention and so on, cannot be studied in the laboratory because the setting up of test cases embodying these processes commits a prohibitive artificiality. In fabricating the behaviour out of context—out of contact with life—it cripples its representation of reality. Hence such results as do obtain from indoor investigation are invalid to the extent that facsimiles are impossible. I have argued that it is not a mirror-image of life which confirms the success of an experiment, but prediction ; and that further, if the laboratory cannot duplicate life, neither does life, as studied *in situ*, ever duplicate itself.

But is not the distinction between laboratory and field science itself largely artificial ? If the *sine qua non* of the scientific method is the selection and discrimination which we call control, then control must be adduced in field study just as surely as in the laboratory. The field student does not just go somewhere and keep his eyes open, as the tourist takes a sightseeing bus unprepared for what is to come. He goes equipped with hunches which he anticipates will be confirmed, hunches which prescribe the boundaries his observations inhabit. He controls just to the extent to which he wittingly rules out factors which might produce changes in the phenomena which he cannot account for. He is not ignorant of these factors. He deliberately holds them in check for the duration of his experiment. He performs a mental isolation—he sees but he eliminates ; he is aware of but he does not attend. If his subject matter is composite, like, say, the contribution of formal education to the socialisation of the individual, he must at least be cognisant of this variousness ; and he must be prepared to alter or dismiss his hunches if what he sees does not mesh with what he expected to see. An interior decorator dresses a room to suit his taste. He supposes that red cushions will be appropriate, or bamboo furniture, or valanced windows. But seeing the room whole, he sees that the appointments he has separately chosen do not balance. He is therefore constrained to alter some of his choices and to substitute others for them which harmonise with the room as a whole. So must the researcher alter his expectations to fit reality as it confronts him.

This implies that he must operate with hunches to begin with. If it is sometimes true that we see what we expect to see, it is also unfortunately true that we do not see what we should see when we have no expectations whatsoever. The field researcher entertains hunches regarding the way the fragments of the world fit together.

He confirms or refutes these hunches. In this manner proceeds the logical tapestry which science weaves.

The procedure is the same in the laboratory, only the reality, the subject matter, does not come ready-made. The experimenter has to create it for himself, and he is limited to the extent that he must fit it within four walls.

But the outdoor-indoor bifurcation is not a significant one. The astronomer cannot bring galaxies indoors ; he must study them as they are, in the field. Nevertheless he is able to perform a great number of experiments. He is able to measure, magnify, analyse, by means of specially devised equipment all focused on the field outside his laboratory. But always he controls—always he limits his field by the hypotheses he has chosen to test, the variables he has chosen to manipulate, ignore, or hold constant, the language he has chosen in which to define and describe. The astronomer can even procure certain fragments of matter for his laboratory. Sidereal bits furnish him clues as to the chemical composition, the nebulosity, or the temperature of the celestial bodies.

Similarly, the psychologist or sociologist cannot bring all the conditions that make for child delinquency into his laboratory, but he can study individual segments of them in the rarefied environs of his special room. In life, these segments would be lost to him, obscured by the illusive transfusion of event and fact, object and change.

The distinction between experimental and field study is a Pickwickian one in respect to created stuffs. The necessity for control wherever research proceeds obviates these differences. The merits and demerits of control in the laboratory invade the field as well. If in laboratory experiments the subject is disarranged by the need to keep some variables constant so that the interplay between others can the more readily be scrutinised, the same is true of the field. The researcher must either immobilise or eliminate factors which he suspects might alter his data or confuse his results, not because he denies their existence or is unknowledgeful of them, but because it is impossible for him to survey a field beyond a previously pared dimension. In one respect, his job is the more difficult of the two. Not himself creating the several conditions of his study, he must attend a hundred influences darting into his circumference. He must hunt for the needle in the haystack by a systematic and thorough search of the field, as a homing pigeon seeks the place of his return by circular gyrations which cover the whole field of his flight.

As much material as can be studied efficaciously at a time can be observed just as adroitly from the vantage-ground of a laboratory equipped to separate the variables of a compound activity, as observed, for instance, directly in a home or school, where environment often obscures relevant variables. An individual under strong affect, whether frustration or volition, passion or brio, exhibits just as genuine a case of ' reality ' studied from a manufactured case as from a natural one. Hothouse rhubarb is still rhubarb. The variables to be held in abeyance may intervene just the same, only the experimenter is aware of them because he has created the conditions which, at his convenience, disclose their dynamics.

But *can* he ' produce . . . states comparable to those which occur in real life ' ? I have claimed that the life-laboratory distinction is authentic only in so far as life in its lived proportions is unique, and in so far as some phenomena are too gross in proportions to be laboratory-confined. It is not authentic if control characterises science. If the field researcher focuses on mass situations he cannot generalise from them, for they never recur. And if in imagination he severs their parts, he is doing mentally what the laboratory researcher is doing in practice, only in the garden instead of the greenhouse. The question is like asking whether the housewife can bake in her own kitchen a cake like the one she ate at the restaurant last night. She can—if she obtains the same ingredients and uses the same recipe. She will gauge her success or failure by the test of taste.

Finally, a contorted result may have the same usefulness as a hypothetical construct. That reality does not reveal its counterpart no more invalidates it than that we cannot find numbers in experience no matter how hard we try.

If the laboratory researcher fails altogether in his simulation, his hypotheses are not likely to be confirmed. Only the test of prediction ever justifies his claim.

As we saw, no activity is in every respect like others, but some are enough alike that we can call them by the same name : rumours, sympathy, compulsions, prejudice, rumination. Likewise, no home-grown experiment is exactly like a life situation, but it is sufficiently alike in factors we can appreciably investigate to warrant our calling it not an artifice but a veritable copy of experience. Certainly as veritable as any copy *within* experience. We cannot study unique happenings scientifically either in the laboratory or in the field. We always fit them into the Procrustean frameworks of our logics, definitions, and hunches, for only these enable perception of similarities and regularities which from a life context seem disparate and unconnected. Without these guides we do not have laws and we do not have science.

At the Fels Research Institute at Antioch University in Yellowsprings, Ohio, an extended investigation on children has been in progress for the past ten years. Field and laboratory workers contribute together to devise and test hypotheses on all factors connected with the growth and development of children—biological, psychological, social. These longitudinal studies, contemplated to range from birth to death, show that field and laboratory can work harmoniously in gathering data, testing assumptions, and prognosticating behaviour. Indeed the distinction between field and laboratory breaks down in this research. The child's home, rigorously analysed from the pre-natal period onwards, becomes the laboratory. The laboratories at the Institute become for a time the home, wherein the child is observed at play and work.

The limits of any research are the limits of what can be brought within our ken. No more can we predict events by stepping into them in all their flux and flow than we can predict the course of a muddy river by wading in it. Whether we must go literally to the field for our data or whether they are amenable to reconstruction within physical confines, we must examine them from the systematic vantage which hypothesis and control effectuate.

Field and laboratory both must finally submit their findings to the predictive arbiter. Only if we fail repeatedly to confirm hypotheses derived from experimental studies may we conclude that the laboratory is ill-adept to explain the group and higher-order processes of man. Field research can contribute to experimentation a diversified panorama of ready materials from which to select data and amplify researches. But these materials lose their distinctness when control, without which no research can proceed, enters into the scientific method to make pliant the obdurate confusion of experience.

In the human sciences one kind of study cannot be pitted against another. Findings and fashions must be shared if progress in this puzzling area is to be made.

London

NOTES AND COMMENTS

A New Interpretation of Gravitation

An important implication of the twin hypotheses of the continuous random origin of matter and of its continuous random disappearance by spontaneous extinction [1] is that these hypotheses might lead to a new interpretation of gravitation. This interpretation is arrived at from the fact that for all regions of space the rate of origin would be constant per unit volume and the rate of disappearance constant per unit mass. The rate of disappearance per unit volume would, therefore, be everywhere proportional to the local mass density. This means that the differential rate of increase of matter, and therewith the local rate of expansion of space, would also differ from region to region. The net rate of increase of matter and the rate of expansion would be greatest in regions most remote from any large accumulation of matter. Within such accumulations, on the other hand, the rate of disappearance would exceed the rate of origin; which might be interpreted as a contraction of space. Between these extremes there must be a given mass density for which the rate of expansion would be zero, and it is relevant to remark that for the cluster of which our system forms a part no expansion has been observed.

Such differential expansion of space seems to involve local departures from Euclidean geometry, departures that would be most pronounced in the neighbourhood of massive bodies. But the only departures that are known are the ones that account for gravitational fields. If the twin hypotheses of random origin and random extinction of matter are valid. one may therefore identify a gravitational field with a region of space subjected to a non-uniform rate of expansion. Gravitation would then not be a property of existing inertial mass, as has always been taken for granted, but a manifestation of the symmetrical impermanence of matter. I say symmetrical because by these hypotheses matter appears impermanent whether one proceeds backwards or forwards in time. A particle of matter may then still be regarded as characterised by two functions, namely as possessing inertia and as the source of an electrostatic field. But there is no need to fasten on it the third function of being the source of a gravitational field. Gravitation, moreover, would be a differential effect, accounted for by the difference in rates of expansion between neighbouring regions, the rate of expansion being in turn proportional to the further difference between the local rates of origin and disappearance. And the faintness of gravitational as compared with electrostatic fields suggests a differential effect, as was recently pointed out to me by L. L. Whyte.

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[1] See R. O. Kapp, *Science v. Materialism*, London, 1940, Chapter XXIV; Letters to *Nature*, **165**, 68, 687; *The Observatory*, June, 1953.

R. O. Kapp

Note on the Structural Philosophy of Organism

Structural biology seeks to account for the properties of organisms in terms of the structure and arrangement of their constituents, such as polymers of protein, nucleic acid, and polysaccharides, as well as other smaller molecules. Attention has hitherto been directed mainly to the equilibrium forms of organic molecules. Their changes of form in course of function have been relatively neglected, though the importance of contractile and flexible molecules has recently been recognised.

A natural hypothesis is that every macroscopic functional cycle is the expression of structural cycles, and that *every persisting molecular structure, when functionally active, undergoes a rhythmic pulsation, in the sense of a deformation from its equilibrium form followed by relaxation.* For example, to every cyclic change in electrical polarisation there may be associated a cyclic change of shape in the molecular structures. Every structure directly involved in the function either of a differentiated system (nerve, muscle, gene, enzyme, etc.) or of undifferentiated cytoplasm may pulsate in course of function, being deformed and then relaxing, either synchronously with its neighbours (e.g. in heart muscle) or in a regular sequence (e.g. along an axon).

There is evidence of such pulsations in the contraction and relaxation (folding and unfolding, or coiling and uncoiling) of contractile protein which appears to underlie[1] cytoplasmic streaming, cyclical changes of polarisation across membranes in muscle and nerve action, and chromosome contraction. Biocatalysts probably pulsate when active. It is reasonable to assume that synthesis and growth occur in steps associated with a structural pulsation. Various types of cells display pulsatile activity.[1] It has also been suggested[2] that pulsations in cortical cytoplasm produce the modifications underlying memory. These cases suggest that *all vital processes may involve pulsating structures.*

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[1] R. J. Goldacre and I. J. Larch, *Nature*, 1950, **166**, 497 ; J. F. Danielli, *Nature*, 1951, **168**, 464

The deformation and relaxation phases of a pulsation combine to produce a closed cycle in a persisting structure which returns to its original state, but they are not exactly the inverse of each other since the first absorbs energy from one part of the environment of the structure, while the second does work on a different part. Every macroscopic functional cycle produces some cumulative change (synthesis, transformation of energy and materials, etc.), and every structural cycle is similarly linked with some progressive transformation in the environment of the pulsating structure. Thus the pulsation of an organic system is not like a reversible mechanical vibration which can occur in isolation, but is the structural equivalent of a thermodynamic cycle and may be compared to the working of a pair of bellows which has a valve open during one phase only and is therefore able to absorb energy and do work, though itself undergoing cyclic changes. But this analogy is over-simplified since some organic pulsations involve helical coiling and uncoiling and produce chiral effects.

There is as yet no general theory of structural pulsations considered as processes involving two interlocked aspects : a one-way transformation of energy and materials, and a cyclic change in a persisting molecular structure. Advance towards such a theory depends on (i) exact determinations of the structural cycles undergone by particular polymers and biocatalysts in course of function ; and (ii) a theoretical examination of the geometrical and energetic or thermodynamic conditions which must be satisfied by all structural pulsations. These tasks may shortly be within reach. The study of equilibrium structures by the use of X-rays, etc. must be extended, if necessary, by the application of new techniques, to determine the exact form of the pulsations of the various organic structures. The current descriptions in terms of geometry (for equilibrium structures) and of chemical concepts (for transformations) must be combined and completed in a geometrical theory of changing molecular structures covering the two interlocked aspects : cycles and one-way transformations.

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[1] See for example C. M. Pomerat, *Journ. Nervous and Mental Disease*, 1951, 114, 430, where other references are given.

[2] L. L. Whyte, *Brain*, 1954, 77, 158

If it is found that all biological systems undergo such structural pulsations in course of function we shall have a clue to the nature of 'biological organisation'. For the structure of every non-pathological biological system must then be such that *the system is capable of undergoing unified pulsations* in which all the parts pulsate either synchronously or in ordered sequence. Many vital phenomena could then be interpreted as the spreading of a characteristic structural pattern as it pulsates, and organisms as self-extending systems of pulsating polymers. But the first step is to establish the exact spatial and temporal form of the pulsations of the various differentiated structures and the general conditions which all pulsations must satisfy.

L. L. Whyte

The Age of the Universe

In his essay[1] Mr Scriven remarks, concerning the respective scientific and philosophical contents of astronomy and cosmology, that 'Gamow and Whitrow today, as Thales and Anaximenes before them, are unaware of the relative proportions of these ingredients'.

Although Mr Scriven gives a specific reference to Dr Gamow, he gives none to me. The unsuspecting reader might easily infer that I subscribe to Gamow's general point of view in the passage quoted from his book, *The Creation of the Universe*. In effect, it is alleged by Mr Scriven that in Gamow's view the philosophical content of physical cosmology is nil. As far as I am concerned, it should be clear both from my own essay[2] as well as from my previous discussion with Professor Bondi[3] that I believe in the contrary view that the philosophical content is of particular significance.

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A Note on 'Degree of Confirmation'

Prof. Popper writes that he has found a simpler formula satisfying the desiderata of his 'Degree of Confirmation' (Aug. 1954): (9.2) is replaced by a formula formed by inserting in the denominator of (9.1) '$-P(xy)$'. The relativised formula (10.1) can be treated analogously by inserting into the denominator of (10.2) '$-P(xy, z)$'.

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[1] M. Scriven, this *Journal*, 1954, **5**, 182

[2] G. J. Whitrow, this *Journal*, 1954, **5**, 215-225

[3] G. J. Whitrow and H. Bondi, this *Journal*, 1954, **4**, 271-283

SYMMETRY

> No, but what is symmetry? Is it not regularity? Regularity then is consistency or agreement or likeness either of a thing to itself or of several things to each other.
>
> The peculiar beauty of the chestnut fan depends (on the fact that) the leaves are pretty much the same but not of the same size . . . although differing they differ by a law diminishing as they do towards the stalk; and this is I presume more beautiful than if they diminished irregularly, so that the contrast of regularity and variety is once more preferred to agreement, the agreement that it would be in this case of entire irregularity.
>
> Gerard Manley Hopkins, *A Platonic Dialogue*, 1865

THERE are notions so general and pervasive, so commonly exhibited and so constantly used that we are apt to take them for granted and dismiss them with a word. Symmetry and beauty are such notions, but whereas beauty can only be vaguely defined, if violently apprehended, symmetry lends itself to a precision which has become more and more mathematical. Symmetry is a character of nature anteceding any human constructions. It is obvious in the flower, the starfish and the snowflake; harder to grasp in the bubble, the fir cone, or the centipede; hidden, until science reveals it, in the diatom, the metal crystal, and the atom itself. Symmetry has also a long history in human material productions, the very first implements, the stone hand axes possess it; the patterns of the first weavers exhibit it to the highest degree; it appears in architecture, in sculpture, and in painting. Transformed to another medium it is manifest as rhythm in dance and music and as rhyme in poetry. But there is a third aspect which appears much later, the conscious apprehension and understanding of symmetry, coming first to our knowledge, with the Greeks and their five regular solids, the origin of so much number mysticism. The full unfolding of symmetry came only with the Renaissance, with Leonardo da Vinci's enumeration of the forms of ornamental symmetry, but most of all from the study and classification of the most regular of the kingdoms of nature, that of minerals and preeminently of crystals. This approach has been from observation, through measurement to mathematical analysis. It is one of the triumphs of human understanding that the complete theory of the 230 possible regular arrangements of points in three-dimensional space should have been reached before a single example of such an arrangement of atoms had been found in a crystal.

Because the fundamental notions of symmetry are purely mathematical it is appropriate that they should be revealed to a general public by one of the greatest mathematicians of our time, Hermann Weyl. His little book on Symmetry,[1] which was the subject of his last lecture at Princeton, is no dry-as-dust treatise, but a most delightful introduction to the most profound ideas. The sequence of four sections starts with the simplest image of the bilateral —right and left—symmetry of the human and animal bodies as exemplified in art. It then proceeds through the radiative symmetry of the flower and the starfish to the repetitive symmetry of geometrical ornament. Each new idea is illustrated by reproductions chosen with rare discernment from the cultures of different ages and peoples and from the world of nature. At the same time the profundities of mathematics are not forgotten. The exhibition of the right and left hands of the human figure leads to a discussion of the fundamental notions of *congruence* and *automorphism*, an operation carrying a figure into another ' indiscernible from it if each of the two figures is considered by itself '. This quotation from Leibniz comes in as part of a discussion on the intrinsic symmetry of spaces, embracing Plato and Kant as well as Helmholtz, Mach, and Pasteur.

In the course of this Weyl refers to Mach's argument about the magnetic needle suspended, above and parallel to the wire carrying an electric current. Symmetry would seem to demand no change of position ; for one direction of deviation is no more probable than the other ; yet the needle does deviate and always in a determinate way. This paradox is resolved, says Weyl, following Mach, because the magnet, round whose atoms the electrons circulate continuously, in one way has no bilateral symmetry contrasting it with the electric current which has. Here surprisingly an error enters which mars the beauty of the argument. True, the magnet is not bilaterally symmetrical, considered electrically, but neither is the current, considered magnetically. The current is surrounded by a magnetic field which circles around it as do the electric currents round the magnet. A transformation of electric to magnetic would transfer one into the other. Here also there is the curious omission of the work of Pierre Curie for it was his consideration of the relations of symmetry of fields that led him to the discovery of the phenomena of the piezo-electricity, virtually the foundation of crystal physics.

These, however, are very minor omissions and blemishes in a treatment that, without any apparent art, leads systematically through the elements of rotational symmetry to their combinations with translatory elements, as for instance in the Greek palmette or key patterns. Weyl unravels the complexities of the Moorish geometrical ornaments and shows that all plane patterns must belong to one of seventeen types. Here one misses a picture showing examples of all these types but throughout Weyl shows himself more concerned with principles than systematic presentation. These

[1] *Symmetry*, Hermann Weyl. (Princeton University Press, New Jersey (Geoffrey Cumberlege, London), 1952. Pp. 163. $3.75 (24s.))

principles include the introduction of the concept of a *translation lattice*, of the matrices that express their transformation and of the quadratic forms that remain invariant by the *group* of these operations. In an appendix he gives a most elegant proof of the classes of finite rotation of groups in three dimensions which includes the generation of the five Platonic solids. The treatment of plane ornament leads to the final complexity of three-dimensional order as exemplified in crystal structures with its 230 different *space groups*. Here Weyl ends his exposition of evident or deducible symmetry and enters on that of his real concern, the fundamental mathematical nature of all symmetry.

First comes a discussion of the symmetries of space and time introducing the basic concepts of relativity and quantum mechanics. In the structure of the atom the permutation possibilities of identical electrons correspond to symmetry in a multi-dimensional momentum space. At this point Weyl leaves the physical world to enter that of pure mathematics with its fields of real and complex numbers and the Galois groups of automorphism, no longer of geometrical points but of algebraic numbers. His last example is the theorem that drew Gauss into mathematics, namely, the constructibility by ruler and compasses of a polygon of p sides, which can only be done if p is a prime number, if it is also of the form $2^{2^n} + 1$, i.e. for 3, 5, 17, 257. . . . This represents a return, with the resources of centuries of mathematics behind it, to the problems that beset Plato and the Greek mathematicians, those of the irrational and the construction of the pentagon.

Throughout, Weyl shows that symmetry is attributable to all 'entities endowed with a structure' even if it is the negation of symmetry, asymmetry, where every part only resembles itself. Of these entities, the simplest and most fundamental is the algebraic field whose elements are numbers.

'What we learn from our whole discussion and what has become the guiding principle of modern mathematics is the lesson : *Whenever you have to do with a structure endowed entity Σ try to determine its group of automorphism.*' The value of an exposition of such a basic notion as symmetry over such a wide field, from art through nature to mathematics is twofold. First, in its ostensible purpose it serves as an introduction and explanation of ideas of increasing complexities, showing to those who have been fascinated by the symmetry they have often seen but never understood. It links the artist, the biologist, and the mathematician. In this it can be compared in profundity, though not in bulk, with D'Arcy-Thomson *On Growth and Form*. It may well lead students, of many disciplines or of none, to explore chosen aspects of this field for themselves.

When, as here, the exposition is carried out by a master it can do more and raise questions which were not before apparent, and whose study can help to create new vistas in science. To the mathematician, perhaps, the book of symmetry is closed once the notion of the group of automorphisms is extracted from it. Compared to this the tedious enumeration of various types of symmetry seems a trivial exercise still more so the consideration of actual examples. Yet for the crystallographer and the biologist it is as important to know why a particular structure or organism shows the symmetry it does as to know abstractedly what symmetry is. How far can these two aspects, the systematic and the formal, be combined ? Weyl's book gives some hints as to the answer of this question. The fundamental formal elements in symmetry are *identity* and *repetition*. From these two all the forms of symmetry in any number of dimensions can be derived. Now in nature identity is the rule for the smallest units—the electron in the atom or the atoms of each element in a compound or crystal. The identity of electrons gives rise to the multi-dimensional symmetries of quantum states or on the three-dimensional level the symmetry of molecules. Already at this level there arises the major problem of the enantiomorphy—right or left handness—of molecules, discovered by Pasteur. In chemical operations, as we perform them, these arise in equal numbers ; in life for each type of molecule only one member appears. The problem of how asymmetry entered into living substance is still unsolved but even without its solution the facts point to the intrinsic unity of life on this planet, the first asymmetry giving rise to all the subsequent ones.

The identity of molecules when packed together in sufficient number gives rise to the symmetry of crystals. This symmetry had always been assumed to be perfect until recent research showed that in a very large number of cases the crystal is only perfect ideally, whereas really it contains geometrical imperfections or singularities, known as dislocations, which play an essential part in crystal growth. The dislocations are not an absence of symmetry but symmetry of a different order lacking the regular lattice translations and unipolar or spiral in character. The relation of dislocations to growth justifies a statement which may have a much wider bearing. A perfect crystal could exist but it could never, as such, come into existence.

The relation of growth to symmetry which begins to appear with crystals is the dominating cause of the visible morphological symmetry of organisms. In the living world we see growth, in relation to internal and external stimuli, moulding the form of the organism and determining its symmetry or more properly its asymmetry. Internally and chemically speaking the cells and simple organisms are, as virtually liquid, completely asymmetrical. Only some viruses and bacteriophages, which as we see them are highly condensed and degenerate organisms, exhibit the regular faces and internal order of crystals. Externally on the contrary, just because of the lack of any fixed internal organisation the simple cell, like any drop or bubble, displays the most complete rotational symmetry—that of the sphere. Any variation of this, whether imposed in single-celled organisms by movement, or in multi-celled organisms by mutual interference, destroys some of this symmetry. Aimless movement leads to a cylindrical symmetry either rotational or spiral ; directed movement to the unipolarity of head and tail. Finally, for a large enough animal, gravity imposes a lower symmetry still—the up and down of back and belly.

When cells in their growth adhere together to form a simple mass they also lose their spherical symmetry but assume the polyhedral shape and still high cubical symmetry of the foam of equal bubbles. Directed growth, as with a plant stem or a muscle bundle, distorts the symmetrical cell into elongated forms of hexagonal symmetry like that of a honeycomb. Two-dimensional extensions like that of a leaf or skin impose further distortions leading to complete loss of symmetry. Yet if the individual cell or even the organism as a whole loses symmetry at one level new symmetry of a different kind may appear at another. In plants concentration of all cell division at growth apices implies a throwing off of subsidiary apices at regular intervals and geometrical conditions limit the angular positions of these to certain ratios which determine the regular appearance or *phyllotaxy* of leaves and buds. This, when extension is held back, provides the apparently radial but really spiral symmetry of flowers. The spiral theme is more evident in the closely bunched units of a pineapple or a sunflower head that illustrate types of repeat symmetry with unequal units just as a crystal does with equal ones.

Radically different and more complicated growth phenomena provide for the apparent symmetry of animals, whether it be of the radial symmetry of the starfish or the repeat symmetry of the centipede. Further growth may modify and destroy this symmetry as when the primitive symmetrical cone of a shell coils itself into a spiral or when the regular phyllotaxy of each single branch results in the complete irregularity of the bush. Internally determined symmetry of this kind indeed must for the whole organism yield to the trimming of external environment and the moulding of function. The oak tree, however irregular its branches, creates a characteristic profile in response to wind and sun. An active fish or animal must keep at least its bilateral external symmetry to swim, run or jump straight, though the internal symmetry of the gut which has no function as such may be totally lost. Externally the ancestral amphioxus type looks like any fish—internally the primitive alimentary canal or the blood circulation of the amphioxus is, in a fish, coiled by extension into forms which have sacrificed their symmetry for efficiency of space filling.

When we come to the symmetry imposed on materials by man the dominant considerations are again different but are not entirely independent. The symmetry of a tool or of a building is in part derived from the imitation of nature, in particular from the most compelling part of nature, the animals on which man lives and the human form itself ; bilateral symmetry, often double bilateral symmetry (orthorhombic polar) is by far the commonest.

The functional aspect in manufacture or use is also a determinant, an arrow head must be symmetrical to fly straight, a spindle whorl must have rotary symmetry to spin an even thread. Yet in artifacts, especially in early times, the dominance of form, aesthetic or magical, imposes more symmetry than is really needed. Early swords have two planes of symmetry, though, as the scimitar shows, one is enough for the blade, while for the most efficient adaptation to the unsymmetrical hand, as the cavalry sabre shows, no symmetry at all is better still.

In the larger constructions—huts and buildings—in early stages at least, bilateral symmetry is functionally imposed. Here gravity comes in, a balance must be struck or the whole edifice collapses. The second, translational, kind of symmetry, that of the arcade, is first imposed functionally in extending the hut to the long house or megaron with its rows of posts, the originals of the pillars of temples and churches. Once established, these two symmetries, the bilateral symmetry of the portico or façade and the translational symmetry of the arcade, are further developed, out of all usefulness, for formal reasons ; they cease to be building and become ornament.

But ornament, as such, long antecedes building. The magical naturalism of the Old Stone Age gave it little scope. It was the basket maker and the weaver who followed her that created ornament in the effort to produce naturalistic figures in the stiff and rectangular medium of plaited twigs. The effort to produce similar figures, such as a frieze of animals, in such a meagre medium led first to identical repetition and then to the appreciation and production of non-representational repeat figures—now mere ornaments—for the sake of the pattern. It was from this new rectangular geometrical ornament that in its turn formal geometry was to emerge. It introduced an additional two-dimensional extension of translational symmetry. The use of colour allowed new rhythms. Indeed, ancient Peruvian textiles show in their patterns even greater variety of complex repeats than are ascribable to any of the seventeen plane groups. The ornamental reed mat of the proto-Sumerian hut is the basis of superficial ornament later to be extended in the more permanent material of the tile. The obligatory fitting together of coloured tiles completed the two-dimensional extension by introducing triangular and hexagonal symmetry, elaborated particularly in the Islamic decoration, which is as far as humanly constructed symmetry can go.

In the graphic and plastic arts, symmetry has had much less place. In fact their development represents a progressive emancipation from its restrictions. The figures of men or animals represented in early Egypt and Babylonia show from the front strict bilateral symmetry and from the side either rigid standing or sitting positions, or the regular repetition of the processional figures each with the leg advanced to indicate movement.

The turning and bending of the body came later by progressive stages. This is not merely a technical achievement, for the Old Stone Age painters had already mastered the most lively and asymmetrical movements of animals, but represents an attitude towards art, depending on its social contexts, from the solemnity and balance of ritual to the freedom and liveliness of individual expression. We see the sequence being played out in one civilisation after another. The Greeks show it from the archaic to the Hellenistic. Then, after a period of aesthetic decay under the Romans, symmetry reasserts itself in Byzantine art and is taken up in the Romanesque, only to bend and twist itself into the realistic and sentimental figures of the late Middle Ages. In our own time, sophisticated with the knowledge of all previous arts, there has been some return to geometric forms but hardly to symmetry which is displayed only in architecture, textiles, and wall-papers.

We can, as Professor Weyl shows in his book, extract from the study of all these natural and human examples of symmetry the fundamental mathematical forms, but this does not exhaust the subject, rather it opens up new problems. Of these almost the most fascinating is how and why we, social human beings, appreciate it or turn away from it, for both may occur at the same time. Something of our appreciation of symmetry must be inborn ; the element of repetition in the external world evoking a resonance in a nervous system responsible to certain rhythms. This is most evident in music but the eye in movement picks up its own rhythms. It can weigh the equality of bilateral symmetry, notably far more effective about a vertical than a horizontal plane. It can circulate indefinitely around a radial pattern. It can follow and extract the idea of movement from a horizontal sequence such as a wave pattern. We can, from some such mechanism, appreciate the beauty of a symmetrical and by contrast of an unsymmetrical flower and later construct according to our skill symmetrical forms that please and move us. These are superficial observations, they deserve the most thorough neuropsychological analyses in which the electroencephalograph may provide the answer. Then will come the more difficult problem of how these innate responses are modified by education and social influences.

The notion of symmetry is indeed inexhaustible, penetrating all fields of nature and art and creating new ones. To focus for a moment on it as Weyl does in his stimulating book brings out at the same time the unity and the movement of nature and the penetration and constructive ability of man in society.

J. D. Bernal

Nature and the Greeks, Erwin Schrödinger (Cambridge University Press, 1954. Pp. 97. 10s. 6d.)

It is a double sign of the times that Professor Schrödinger should feel the necessity in 1945 for prefacing a course of public lectures on nature and the Greeks with 'ample explanations and excuses', and that when speaking on the same topic at University College, London (Shearman Lectures) in 1948 he 'already felt much more self-assured'. In the first attempt to give an ordered account of the nature of things there would be no concepts ready at hand in which the terms of the account could be expressed. An important function of subsequent Greek thought was the examination of these conceptual terms to discover their implications, their suggestiveness, their internal inconsistencies. The same sort of thing also happens whenever, in Whitehead's phrase, 'you tamper with your basic concepts . . . philosophy is then merely the marshalling of one line of evidence and can not be neglected'. That this is implicitly Professor Schrödinger's view also is evidenced by his reference to 'the inordinately critical situation in which nearly all the fundamental sciences find themselves ever more disconcertingly enveloped'. But his 'excuse' for a return to Greek ways of thought contains another quite different element, namely, that 'never before or since, anywhere in the world, has anything like their highly advanced and articulated system of knowledge and speculation been established without the fateful division which has hampered us for centuries and has become unendurable in our days'. These are strong words, strongly felt: and they refer not only to the divorce between science and philosophy, but of both from religion. 'Science suffices to jeopardise popular religious convictions but not to replace them by anything else. This produces the grotesque phenomenon of scientifically trained, highly competent minds with an unbelievably childlike—undeveloped or atrophied—philosophical outlook.' In setting forth his views on the relevance of Greek natural philosophy to these issues Professor Schrödinger does not follow the usual chronological order but discusses the Greek contribution under the headings *Reason* v. *Senses*, *The Pythagoreans*, *The Ionian Enlightenment*, *The Religion of Xenophanes*, *Heraclitus of Ephesus* and *The Atomists*. In each of these, suggestive and, probably to most readers, novel aspects are brought to light, as in the reference to the fragment of Democritus implying doubt as to the materialist theory of sensation, and again in the emphasis on the rotation of the earth *implicit* in the Pythagorean system. Nevertheless, the reader must be on his guard against such unsupported generalisations as 'They [the Ionians] take the world as given to us by our senses and try to explain it, not bothering about the precepts of reason any more than the man in the street does, from whose way of thinking theirs is a direct descendant. Indeed it frequently starts from problems or analogies of handicraft and serves practical applications in navigation, mapping, triangulation' (p. 51). The surviving fragments give us no warrant for any of these statements—did the senses suggest Anaximander's drum-shaped earth or the 'breathing holes' through which the fire of the heavenly bodies is seen? On the other hand, what they *are* concerned with is the origin of the world—hardly a problem likely to arouse the enthusiasm of a handicraftsman or navigator. Professor Schrödinger barely alludes to Greek medicine; but it is a significant fact that the author of *Ancient Medicine* warns off those who 'have assumed for themselves a postulate . . . heat, cold, moisture, dryness or anything else that they may fancy—who narrow down the causal principle (ἀρχὴν τῆς αἰτίης) of diseases and death among men, and make it the same in all cases . . . ', and just for the very reason that medicine is a craft (τέχνη) which has no need of an 'empty postulate' (κενῆς ὑποθέσιος). It is a pity that Professor Schrödinger did not consult the works of Cornford—especially the posthumously published *Principium Sapientiae*, in which the latter made out a strong case for the origin of Ionian speculative natural philosophy from the creation myths of Babylonia—probably *via* the *Theogony* of Hesiod. Here Professor Schrödinger would have found a detailed picture of the unity of religion and natural philosophy in Greek thought such as his own book seeks but hardly provides. This is not to say, however, that *Nature and the Greeks* is not a book of great charm and one which points to a new approach to the solution of the cultural schizophrenia of our time. The note on which it closes is the claim that in Heraclitus there appears for the first time that double-edged weapon of the elimination of the subject from the scene of intellectual appraisement, whereby science is made possible but devoid of human content. It is to be hoped that Professor Schrödinger may develop this challenging idea.

I noted only one misprint—'Proust' (p. 57) should be 'Prout'.

William P. D. Wightman

Nomological Statements and Admissible Operations: Studies in Logic and the Foundations of Mathematics, Hans Reichenbach. (North-Holland Publishing Co., Amsterdam, 1954. Pp. 140. £1 7s.)

In actual usage we find little difficulty in distinguishing between 'reasonable' and 'unreasonable' implications. The implication *if the metal is heated, it expands* will undoubtedly be regarded as reasonable, whereas to say that *if snow is not white, sugar is sour*, although true in virtue of the false antecedent, strikes us as unreasonable.

Implications, and also statements obtained by other propositional operations, are widely used to express laws of logic and laws of nature.

The author includes such statements under the name of *nomological statements* and the problem he proposes to investigate in the monograph, is to find out what requirements must be satisfied by a nomological statement if its main propositional operation, referred to as a major nomological operation, is to be reasonable.

The general form of the theory suggested by the author can be outlined as follows. First, a class of *original nomological statements* is defined by setting up a number of requirements to be satisfied by a member of the class. Then the class of *nomological statements* is constructed to comprise all those statements that are deductively derivable from sets of statements of the first class. Among nomological statements a narrower group of *admissible statements* is singled out. This group is regarded as supplying reasonable propositional operations while the class of nomological statements is supposed to supply the laws of nature and the laws of logic. The problem of conditionals contrary to fact is investigated at length within the framework of the theory, and a brief account of the methods of inductive verification in their relation to all-statements is given in the appendix.

The monograph has been written with the intention of developing an amplified and improved version of the theory presented by the author in his *Elements of Symbolic Logic*, New York, 1947, Chapter VIII. Although the present contribution is primarily concerned with uncovering rules which govern propositional operations in a natural language, it will not make easy reading for anyone who is unacquainted with symbolic logic in general and with the author's *Elements* in particular. The theory is rather technical, and its presentation involves a great number of terminological conventions (forty-nine in all) the significance of which is at first not easily seen. The author rightly remarks that in searching for a definition of reasonable operations as used in non-formalised languages one cannot arrive at results which leave no room for discrepancies between the *explicans* and the *explicandum*. But he seems to be justified in claiming that the definition he has proposed, corresponds to ordinary usage at least in a high percentage of cases.

Formula (21), page 29, ought to read: $a.b.c \vee a.b.\bar{c} \vee \bar{a}.\bar{b}.c \vee \bar{a}.\bar{b}.\bar{c}$; formula (75), page 77, ought to read: from '$a.c \supset b$' to '$a.\bar{b} \supset c$'.

Czesław Lejewski

Dimensional Methods and their Applications, C. M. Focken, with a Foreword by H. Dingle (Edward Arnold, London, 1953. Pp. viii + 224. 30s.)

In recent years there has been a spate of books on the method of physical dimensions but in view of the great confusion of opinion concerning the nature, scope and validity of the method it cannot be claimed without careful prior examination that any new addition to the literature is redundant. Indeed, this latest addition has distinct merits of its own and is clearly the product of erudition and careful thought, despite the serious objection raised below.

The author, who is Director of the Museum of Applied Science of Victoria and formerly Reader in Physics in the University of Otago, acknowledges the influence of Bridgman and Dingle on his views. He quotes a penetrating statement by the former that ' dimensional analysis is essentially of the nature of an analysis of an analysis ', and he follows the latter in insisting that dimensions characterise magnitudes, i.e. the results of measurements of physical magnitudes, and are not to be regarded as intrinsic properties of physical objects. As Dingle indicates in his brief Foreword, the realisation of this vital distinction was one of the most important consequences of the theory of relativity.

The author begins with an elementary but stimulating account of the fundamental ideas of the subject, in particular *units*, *dimensional homogeneity* and *principles of similitude*. He illustrates these with a number of useful examples, marred by an unfortunate slip (p. 13) where the term ' moment of inertia ' appears instead of the correct expression ' radius of gyration '. He enlivens what could easily be a tedious catalogue by imaginative comments ; for example, when introducing the principle of similitude he shows that we can deduce that the larger an animal the less its cooling surface compared with its size and then remarks : ' So the principle of similitude enables us to show that a very small animal needs relatively more food than a big one to keep it warm, and therefore helps to explain why small animals are not indigenous to the polar regions.'

The second chapter is devoted to the formulation of the basic principles of dimensional analysis leading up to Buckingham's Π Theorem and illustrations of its use. Despite the pains which the author has taken to explain this theorem, some points of obscurity still remain. For example, he claims that ' the numeric of a physical magnitude can be operated on in the same way as a number ' (p. 44). This statement, however, is either a tautology or else false, for a numeric is either a *number* or else it is not. The essential point, which somehow never seems to be clearly stated, is that the algebra of physical magnitudes is *not* the algebra of ordinary numbers. Although the ordinary addition law (Tolman's criterion of extensivity) is usually obeyed (but not invariably—cf. the velocity-addition law of Special Relativity) so that, for example, we can *add* a length to a length to get a further length—their ' sum '—if we *multiply* a length by a length we do not get a further length but a different kind of physical magnitude altogether. Thus, when the author states that

$$v + s = at + \tfrac{1}{2}at^2$$

is a typical example of an equation which is 'physically meaningless' although 'mathematically correct' he is guilty of a serious mistake. Such an equation is, in fact, *mathematically incorrect*, since the operation + has not been defined for physical magnitudes of different dimensions. The equation would be mathematically correct if v and s were regarded as dimensionless numbers, but this is not what the author intends.

In the third chapter the dimensions of various physical magnitudes are discussed, particular attention being paid to the question whether in addition to the three fundamental dimensions of mechanics further fundamental dimensions should be contemplated, for example *charge*. The two remaining chapters are devoted respectively to applications of dimensional analysis to particular problems in physics and engineering.

A notable feature is the useful collection of problems for the student to work for himself. Answers are printed at the end of the book.

G. J. Whitrow

Dilemmas, The Tarner Lectures 1953, Gilbert Ryle. (Cambridge University Press, 1954. Pp. 129. 10s. 6d.)

In each of these dilemmas we are confronted with two seemingly contradictory statements neither of which we see how to reject. Professor Ryle sets out to resolve our difficulties and to bring to light the sources from which dilemmas spring by analysing the behaviour of concepts. In any field of activity there is a cluster of interrelated concepts—such as trump, slam, revoke in Bridge, whole, part, add, multiply in Arithmetic, event, before, after, cause in relation to occurrences, truth, implication, necessity in Logic—which we are able to use without difficulty so long as the rules for their use are familiar. In unfamiliar fields the old rules may let us down and we help ourselves out by following rules proper to other sets of concepts, and this is a fruitful source of perplexity.

The dilemmas discussed concern Fate, where the concepts of Logic are mingled with those proper to occurrences; Achilles and the Tortoise, where the concepts of Arithmetic are made to function queerly in conjunction with those belonging to racing; Pleasure, where the concepts applying to pains and sensations are brought into a field inappropriate to their use; the relations between the World of Science and the Everyday World, where concepts of physics, logic, physiology, and psychology combine to produce a complex tangle; and finally, the relations between Logic and Philosophy.

Consider his treatment of Achilles and the Tortoise. We know from simple Arithmetic where Achilles will overtake the tortoise. Yet Zeno manages to perplex us through the way in which he puts the problem. Achilles must first reach the position the tortoise starts from. Meanwhile the tortoise has reached a new position. And this goes on happening. It looks as if Achilles must at some stage *first* get to where the tortoise was and *then* get to the point the tortoise has gone on to, *without* letting the tortoise get a bit further on. Unless he does this he will never catch the tortoise. And Zeno challenges us to show in detail just how this is done.

Professor Ryle's main line is to point out that while Zeno gives us enough evidence to show that there is a total stretch between the starting point and the end of the race, yet by the way he puts the problem he divides up his total stretch in such a manner as to ensure that there is always a bit left over. Ryle compares Zeno's device to that of a woman who hands round a cake with the instruction that each person is to take just half of what remains on the plate. The entire cake will never be taken, but there is nothing mysterious in this. Why are we perplexed in Zeno's case? Ryle says that by inducing us to look at the race with the eyes of a runner uncertain whether he will win, Zeno is blinding us to the fact that there is an entire stretch. All the parts mentioned in the problem are parts cut *off* the total course, but the total course is not cut *into* these parts. Once we see that there is always a part of the total course left we see that it is Zeno's mode of procedure that makes the difficulties. We were wrongly tempted to attribute to the course what belongs to the procedure. Because in Arithmetic $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} \ldots$ will never make 1 we were led to conclude that in the racecourse Achilles will never catch the tortoise. Here are two entirely different senses of ' never ', and we were hypnotised into neglecting their difference. Again, at any point in the race all the parts so far cut off the total course make a countable total, and however far we go all the cuts leave some of the course still to be gone through. But this last ' all ' means ' any, no matter which ', and we haven't a countable total, nor an uncountable total ; and part of our difficulty lies in our having to operate with both kinds of ' all ' at the same time.

While I think Ryle's treatment takes us some way with the problem I don't think it takes us the whole way. Zeno's point is that when Achilles gets to the place from which the tortoise started, all he has done is to put himself in precisely the same position as he was in at the beginning of the race, though the racecourse is now smaller, and that this situation apparently gets repeated indefinitely. Zeno wants to know how, this being so, Achilles can ever win. And even after Ryle has done all he can to persuade us that Zeno's way of putting the problem is illegitimate we remain not quite convinced, and want Zeno's challenge to be met more on his own ground.

The conflict between the physicist's account of the world and our everyday account turns on the status of secondary qualities, colours, sounds, etc. Because his account of the properties of matter does not mention them we are tempted to believe that they are not properties of things at all.

Ryle meets this by discussing the various ways in which things can have properties. Being worth a shilling, being red, being spherical are different kinds of property. And just as an accountant can give a complete account of the financial aspect of a business without mentioning a host of other aspects, so it is with the physicist. The fact that he only mentions primary qualities does not show that these are the only kinds of quality things can possess. We begin to clear up our perplexities when we turn our original problem into the problem how the concepts of physical theory are related to the concepts of everyday discourse.

But this does not clear up the matter entirely. For the physiologist, regarding perceiving as a physiological and perhaps partly as a psychological process, concludes that things cause us to have perceptions of colours, smells, tastes, etc., without having these qualities themselves. Ryle contends that what is wrong here is the assumption that perceiving is a process. The verb ' to see ' is not like the verb ' to run ' but like the verb ' to win '. You can't ask how long a man's seeing took. Seeing marks the successful termination of a process. Ryle puts this by saying (p. 103) ' I can be looking for or looking at something, but I cannot be seeing it '. This sounds odd. But even if it is right, does it settle the matter ? Ryle rejects the arguments of the physiologist and the psychologist on the general ground that perceiving is an achievement and not a process. But the question still remains, how these arguments affect looking at and observing, which are processes, and whether the physiological and psychological accounts involve the conclusion that what we see is after all so affected by the conditions of our observing as not to be attributable to things in the absence of a perceiving organism.

Professor Ryle's treatment of his dilemmas is stimulating and provocative. It is not vital to his task that he should convince us that he has resolved all the difficulties. What he is doing is to show how philosophical enquiry is appropriate to situations where the clearing up of perplexities does not depend on our getting further facts but on more careful use of our concepts. And this he does admirably.

L. J. Russell

Reason and Nature : an essay on the meaning of scientific method, Morris R. Cohen. (The Free Press, Illinois, 2nd ed., 1953. Pp. xxiv + 470. $6.00.)

This new edition of a work which will be familiar to most readers of this *Journal* includes, Professor Northrop states in a Foreword, ' all the detailed corrections which the author incorporated in his own copy '. I have not had the leisure to make a comparison with the first edition and the alterations are not indicated in the text ; but they do not appear to be substantial.

Re-reading a book which one read with a sense of revelation as a student can be rather like re-visiting a house one knew as a child : one finds it curiously diminished and faulty. I found, for instance, that Cohen lacked scrupulous fairness in dealing with his opponents' views (especially Kant's). To give one example : he dismissed the view that biological phenomena differ from physical phenomena in depending on their past with the remark that the working of a machine depends on its age. Anyone who could be so easily refuted would be a fool. What Cohen needed for a genuine refutation is two *similar* physical systems which behave differently in similar circumstances because of their different histories.

On occasions he could be very vague. This is his strange conclusion on the vitalist-mechanist controversy in biology : ' despite the association of vitalism with a hazy idealism (which is really subjectivism and nominalism) mechanism is much more in harmony with true objective idealism '.

Although this is a long book, some of the discussions are brief and superficial. This is particularly so with determinism and the mind-body problem. Cohen rejected two-way interaction with the characteristic remark, 'To attribute physical causation to purely mental elements is to open the flood-gates on all sorts of superstitions.' It would be curious if a man who devoted a book to the defence of Reason really believed that thinking cannot affect anything, and in fact Cohen did not adhere to his epiphenomenalism. Indeed, he criticises social determinists for inconsistently supposing that their own knowledge has any practical importance.

His chapter on psychology is notable for his contempt for psychoanalysis and for his flat rejection of the unconscious : ' between bodies and consciousness there is no third intermediate entity '. He did not bother to discuss Freud's ideas, and one is painfully reminded of other philosophers of science like Bacon and Comte who did their best to stifle young enquiries which ran counter to their prejudices.

But his dogmatic scepticism could be fruitful, for instance in connection with induction which he regarded as a myth and replaced by the hypothetico-deductive interpretation of scientific procedure. What he says about method was in advance of his time and is well worth reading today, though he had not seen all the implications of his position. He allowed the invention of *ad hoc* hypotheses to explain awkward facts ; and he spoke of ' definitely established and verified laws of natural science ', apparently supposing that the number of possible rival hypotheses is finite and may be as few as two. He also associated the scientific outlook with doubt and uncertainty. His attitude here is reminiscent of the theological idea that only someone who has passed through despairing disbelief can achieve true faith.

However dissatisfied and even irritated one may be with certain passages in this book one recognises a certain grandeur about it. There is its critical scope : mathematics, physics, biology, psychology, the social sciences, history, political theory, jurisprudence, ethics. And there is the fact that it is through and through a *moral* work, the expression of a sad, stoical, near-religious philosophy of life in the technical terms of a philosophy of knowledge.

J. W. N. Watkins

Berkeley, G. J. Warnock. (Penguin Books, Harmondsworth, Middlesex, 1953. Pp. 252. 2s.)

This is a welcome addition to the useful Pelican Philosophy Series, edited by Professor A. J. Ayer, for Mr Warnock succeeds admirably in giving a clear critical account of Berkeley's work in some two hundred and fifty pages.

He summarises Berkeley's theory of material existence—*esse est percipi*—in the following propositions : (i) if there are no ' external bodies ', there are only ideas ; (ii) if there are no ' abstract general ideas ', objects must be collections of *particular* ideas ; and then comments : ' In order to escape from this straight-jacket it is essential to recognise the possibility that Berkeley could be correct in what he denies, without necessarily being correct in what he asserts.'

Berkeley's theory of existence embraced ' spirits ' as well as ' matter '. Indeed, his theory of the latter entailed as its correlative, when the abstract general idea of existence was rejected, the no less peculiar doctrine that the *esse* of ' spirits ' is *percipere*.

Although the most conspicuous, Berkeley's existential statements are not the only ones which puzzle the student. Warnock places his finger firmly on the root difficulty, namely, Berkeley's axiomatic belief that our ideas are of ' sensible qualities ', so that he had to interpret all statements which do not ascribe qualities to things (e.g. existential, numerical and relational statements) as recording the operations of a spirit, although each such interpretation resulted in ' intolerable paradox and oddity '. Nevertheless, Berkeley's analysis of these problems was not without value. He saw that ' what makes a general term general is not that it stands for a special kind of idea, but that it is *used* in a particular way. . . . But it seems not to have occurred to him to ask quite generally of any word " How it is used ? " '

As Warnock goes on to point out, although the question which Berkeley asked instead (To what *particular* ideas do we use such-and-such a word to refer ?) led him astray in discussing existence, number and relation, it served him far better in his discussion of motion. This topic is included in the single chapter of this book which the author devotes to Berkeley's famous criticisms of science and mathematics. Although like most general philosophers, Warnock regards these as constituting only a minor part of Berkeley's claim to fame, he lays great emphasis on his insight and originality in *De Motu*, where he drew a fundamental distinction between the observed facts of science and the theories constructed to comprehend them.

In the final chapter, Berkeley's 'ideas' and theory are compared with modern 'sense data' and phenomenalism. The author follows contemporary fashion by drawing from his critique of Berkeley the moral that the proper task now facing philosophers is to investigate 'the immensely complex vocabulary of perception in ordinary language'. Philosophers of science, however, will perhaps feel more inclined to draw the rather different conclusion that the study of language (by philosophers as distinct from philologists) is in danger of degenerating into mere logomachy and verbiage unless due account is taken of the *purposes* of language.

G. J. Whitrow

Homage to George Berkeley (Hermathena. Published for Trinity College, Dublin, by Hodges, Figgis and Co., Dublin, 1953, No. 82. Pp. viii + 146. 10s.)

This volume contains the papers read during the bicentenary commemorations at Trinity College, Dublin, in July 1953. There is a lightness of touch, befitting a festive occasion, about most of the papers, which deal with various sides of Berkeley's activity, as a man (Professor Jessop), as a philosopher (Professor Luce), as an economist (Professor Johnston), as critic of religious thought (Canon Ramsay), and as critic of contemporary science (Dr Whitrow). Professor Dobrée analyses Berkeley's style, showing something of the secret of its musical quality. Professor Leroy shows the reactions to Berkeley's ideas in Europe, while Mr Popkin studies their influence on American philosophy.

The paper which will be of most direct interest to readers of this *Journal* is that of Dr Whitrow, on Berkeley's Critique of the Newtonian Analysis of Motion. Dr Whitrow deals with two points, Berkeley's attack on the logical principles underlying Newton's doctrine of evanescent quantities, and his attack on Newton's concept of absolute motion. Taken together the papers present an admirable picture of Berkeley's many-sided genius.

L. J. Russell

The Science of Mind and Brain, J. S. Wilkie (Hutchinson's University Library, London, 1953. Pp. viii + 160. 8s. 6d.)

This small book presumably is not written for the professional neurologist or psychologist. On the other hand, it provides the 'intelligent layman' with a sound introduction to many aspects of the mind-brain problem.

It is good to find that Dr Wilkie is not misled by some modern philosophers who would deny the private ' mental world ' any place in a philosophical and scientific investigation. But the reviewer would have liked to have seen a more systematic treatment of the problems that arise in relationship to consciousness, free-will, memory and the self. Unfortunately Dr Wilkie largely restricts his psychological treatment to perception.

Macroscopic anatomical information occupies a disproportionate amount of space in view of the fact that it helps so little in our understanding of the working of the brain and of the mind-brain problem. In particular the two chapters on the size of the brain are virtually irrelevant to the main theme, and could be made relevant only by discussing the fine structure of the brain. For example, the potential performance of the brain presumably is related to the aggregate of the functional connections that can be made by the constituent neurones, and hence, other things being equal, to the number of constituent neurones. Thus the potential performance of the brain would be more significantly related to the number of constituent neurones than to its mass.

There appears to be a further deficiency in the scant reference to the insight that electrical recording gives to the working of the brain. Already the results are of the greatest interest and there can be no doubt that herein lies the most fruitful line for future investigation.

However, these criticisms concern merely the relative emphasis to be placed on various investigations relating to mind and brain. Dr Wilkie has chosen his factual material well and has displayed sound judgment in his critical appraisal. Particular commendation should go to his treatment of the gestalt problem. His final conclusion is that ' The final integration of the neuromental system may take place according to laws which are unique, and it is possible we shall never know them otherwise than by the psychological methods of introspection and behavioural studies '.

This is a book that can be thoroughly recommended to the intelligent layman.

J. C. Eccles

A Logical Analysis of the Theory of Relativity, Håkan Törnebohm. (Almquist & Wiksell, Stockholm, 1952. Pp. 273. Sw. Cr. 25.)

In the first, and larger, part of this volume the author describes, in technical and mathematical terms, the main results of both the special and the general theory of relativity. In the second part he examines the logical character of the concepts and of the structure of the theory.

The account leans strongly on the, by now classical, exposition which was given by Reichenbach in his *Philosophie der Raum-Zeit-Lehre*. However the operationalist view is emphasised here though its limitations are noted. The author speaks, for instance, of the theoretical or constructive definition of ' length ' and of its operational definition, saying that ' the former . . . has a regulating function in relation to the latter . . .'. What this distinction is does not come out very clearly. A remark in a footnote seems to indicate that the distinction is similar to that between a syntactic and a semantic definition. But the question remains, What is an operational definition ? What we do in the laboratory does not uniquely explain the meaning of a statement ; we cannot accept Bridgman's dictum that we have as many meanings to a sentence as we have operations. Moreover, we must first know at least *a* meaning before we can devise an operation for elucidating it. The essential difficulty is always to find out how what we do relates to what we say ; and this presupposes that we have already 'semantised ', or given a meaning to, some of our operations.

Five different levels of discourse are supposed to play a rôle in science. These are the levels of sense-data, of things, of empirical or operationally defined concepts, of theoretical (' constructive or constitutive ') concepts and, finally, of a still higher schematisation. The sense-data terminology belongs to the philosophic doctrine of empiricism and certainly has no application in science. The author recognises this to some extent by admitting that ' test propositions can never exclusively be expressed ' in terms of sense-data. (But sense-data were invented exactly for the purpose of making sentences testable !) The step from the third to the fourth level is claimed to represent the beginning of a scientific theory : it is a ' linguistic transition ' from things to physical objects. Is this enough for characterising the relation between the various discourses ? And what is the difference between a thing and a physical object ? This seems to me to introduce all sorts of queer ontological assumptions.

The many rôles laws play in a scientific theory are then explained by reference to the different levels to which they belong. Usually, a law is part of what is called here the *descriptive matrix* of a theory, and so it is not to be regarded as an isolated, factual, sentence. Similarly, functors like ' position ' or ' velocity ' require a co-ordinate system ' in relation to which . . . the functors take their values '. This exemplifies the fifth level of theory construction, namely, the level given by functors which are *closed* with respect to a chosen frame of reference. The author thus arrives at the result that ' the theory of relativity is essentially a reconstruction of classical physics based on new closure rules '. It is therefore a mistake to forget these rules, as we do when we give the new functors in relativity theory the same name as those in Newtonian mechanics, for they have not the same designation.

There are interesting ideas to be found in this book, e.g. in the discussion about functors ; but there are objections to be made as well, especially about the author's view of causality ; and the chapter on dimensional analysis is rather obscure. The explanations given here do not go essentially beyond what has been said before, and this is so because the author clings too much to traditional epistemologies instead of leaving them behind—a lesson which, I think, relativity has taught us.

The changed conceptions about theory construction which relativity theory has brought about are today largely forgotten, I suppose, because the changes introduced by quantum theory are so much more striking. This book will help us in turning back our attention to the methodological problems of classical theory which have been so sadly neglected.

E. H. Hutten

Substance and Function and *Einstein's Theory of Relativity*, Ernst Cassirer. Authorised trans. by W. C. and M. C. Swabey. (Dover Publications, New York, 1953. Pp. xii + 464. $1.95.)

This is a re-issue, in a single volume, of two books the translation of which was originally published in 1923 ; the German text of the first book dates from 1910, and the second work appeared in 1921.

Both books are widely known and a detailed review of what they contain is hardly required. The late Professor Cassirer belonged to the Marburg school of neo-Kantianism though, by the breadth of his scholarship, he transcended the limitations of this doctrine. All the same, the position taken here is that of classical philosophy and of epistemology. There is the claim that philosophy is some sort of super-science which lays bare the foundations of science as well as of ethics ; and that we must have a theory of knowledge —separate from, and over and above, science—in order to justify it. An illustration may be given by the statement, e.g. that ' the ideality of the forms and conditions of knowledge, on which physics rests as a science, both assures and grounds the empirical reality of all that is established by it . . .'.

This *cognitivism* arises from an ontological assumption about the Nature of nature and of man. If we ask, with Kant, How is knowledge possible ?, we have already introduced a dichotomy and then must invent a special process of cognition by means of which we can connect the facts outside our heads with the ideas inside them. It is the classical view inherited from the Greeks, though Kant brought it up-to-date in order to make it conform, as much as it is possible, with Newtonian mechanics.

Today there are very few philosophers, and still fewer scientists, who would openly adopt this view (but they do not always succeed, unfortunately, in adjusting their eyes to the different focus required by modern science). A change in attitude has occurred about what constitutes a problem and how to solve it, a change similar to the one which, for example, transformed alchemy into chemistry. We no longer think that we must justify *that* we know ; instead, we try to reconstruct in logical terms the actual theories of science. And in order to explain *how* we know we have recourse to modern psychology, instead of inventing philosophical theories about the human mind. For behind all such theories lurks the mind-body problem ; and these represent futile attempts of trying to put mind and body together again after they have been severed by a neat, though often hidden, ontological incision. This is shown up by the various dichotomies in terms of which the discussion here proceeds, e.g. of abstract *v.* concrete, subjective *v.* objective, and phenomenon *v.* reality (with a capital R). Obviously, such philosophical theories are truely meta-physical, that is, outside the domain of science ; and, consequently, the terminologies proposed by them are not very useful in explaining a scientific theory. Rather, the opposite is true, namely, that the astonishing theories about mind and body which philosophers have brought forth in the course of the last two thousand years may themselves be taken as objects for scientific study. It might make us understand why these philosophies—like our dreams and phantasies—have had (and occasionally still have) so strong a hold over us.

Though the kind of philosophy which Cassirer brings to the problems of science has no longer much force, it would be wrong not to appreciate the historical merit of his views and to overlook the suggestive value they still possess. And the great clarity of his exposition is always enjoyable. Certainly, this volume should, and I hope will be, read by everyone interested in the philosophy of science. These books belong to the classical literature on the subject. The publishers deserve our thanks for having made them available again, and at a reasonable price.

Ernest H. Hutten

The Works of Archimedes, edited in modern notation with introductory chapters by T. L. Heath, with a supplement, *The Method of Archimedes* (Dover Publications, New York, 1953. Pp. clxxxvi + 377. Cloth. $1.95.)

A hearty welcome must be extended to this reprint of Heath's classic book on Archimedes, first published in 1897 by the Cambridge University Press, particularly as it incorporates the 1912 supplement on *The Method*.

The long introduction contains a brilliant survey in eight chapters of the life and work of Archimedes, the manuscripts, principal editions and terminology. The main body of the book comprises translations of his extant works, together with a chapter by Heath on the Cattle Problem.

Archimedes ranks, by common consent, with Newton and Gauss as one of the three greatest mathematicians the world has ever seen. Unlike

Euclid and Apollonius he did not systematise and generalise the methods used by earlier geometers. Instead, in all his extant works, he is completely original. For example, earlier geometers attempted to square the circle without much success, whereas it occurred to Archimedes to try to square the *parabola*—a problem which he not only attempted but solved. The method of exhaustion, invented by Eudoxus, became in his hands an anticipation of the integral calculus. He was the Newton of the ancient world. But the parallel with Newton runs deeper. Although both wrote their works in the style of Euclid, each had a secret method of discovery (which he distinguished from the method of strict proof)—in Newton's case the calculus of fluxions and in Archimedes' a mechanical method. In this connection one of the most important finds of the last fifty years concerning the ancient world was made by the scholar J. L. Heiberg in 1906 when he recognised that a palimpsest in a Constantinople monastery contained among other works of Archimedes the lost treatise known as *The Method*. The original parchment had been used in the tenth century to copy out these works, but some two or three hundred years later an attempt was made to wash out the old writing so that the parchment could be used for writing a Euchologion. Fortunately, the attempt was only partially successful. The Archimedian 'method' thus revealed was an ingenious instrument of mathematical investigation based on the theory of the lever, and today its study must be regarded as an essential part of any comprehensive course on the history of scientific method.

G. J. Whitrow

Bertrand Russell's Theories of Causation, Erik Götlind (Almquist & Wiksells Boktryckeri AB, Uppsala, 1952. Pp. 164. 16 Kr.)

The aim of this book is to set out in chronological sequence the development of Russell's ideas about causation, from the *Principles of Mathematics* to *Human Knowledge*, and to correlate Russell's changes of opinion on this subject with changes in the theoretical structure of physics during this period. It is proposed therefore to show the impact of relativity-theory and quantum mechanics on a system of philosophy conceived within the matrix of classical physics but sensitive to changes in its scientific environment. So far as this is a feasible scheme, it is competently carried out, though without, I think, throwing any fresh light either on Russell's philosophy or on the logical problems involved. The execution of this programme necessarily diverges somewhat from the advertised plan, since Russell's preoccupation with traditional problems of epistemology is not deeply affected by changing theories of physics : for instance, his attitude towards the justification of induction and his late dissatisfaction with the view that causation is merely invariable sequence. Mr Götlind's discussion of eseth questions is unsatisfactorily cursory ; it is significant that Hume's name does not appear in the index. Nor is the author's exposition always easy to follow, without a copy of Russell before one : this is due partly to bad arrangement (for instance, discussion of the five Postulates in *Human Knowledge* begins before these have been explicitly stated), partly to the writer's difficulties with the English language, in translating the original Swedish manuscript. It is doubtful, too, whether the chronological method is well chosen ; it makes for repetitiveness and irritating cross-references, and a loss of thematic continuity. Misprints abound ; even the correction slip requires correction. Most of these errors are of too crass a quality to be seriously misleading.

R. J. Spilsbury

The Mind and the Eye, Agnes Arber. (The University Press, Cambridge, 1954. Pp. xi + 146. 16s.)

Perhaps it is a presumption, but the reviewer feels on reading Mrs Arber's contemplation on some theoretical aspects of biology that she must be a very charming lady. Seldom has one read a book which is profound, very beautifully written and which has given so much pleasure. It is not the sort of book one wishes to criticise, although many biologists who are versed in modern methodological outlooks in science will find plenty to disagree with in it. Yet as a whole there is evidence of scholarship and depth of wisdom.

The book is divided into two parts. The first is an account of five stages which are passed through in the acquisition of biological knowledge. The writer discusses the reasons for choosing problems and then describes the general method of biological discovery. Mrs Arber is inconsistent on one point, however, for she says on page 17 that in making additions to purely factual knowledge the mental aspect is relatively unimportant, and yet later in Chapter X she is careful to point out how humiliated she was when a purely factual observation escaped her for a long time because she was not mentally ready to become aware of it. Further chapters are devoted to logic, the use of analogy and the art of writing up one's results. It is a pity that no account of the contributions of modern scientific philosophy and logic has come Mrs Arber's way for her third chapter is weak because of this. In describing the art of biological writing, an art in which she is obviously an expert, Mrs Arber says some very interesting things. She does not want writing to be too brief or too lucid, for as she points out over-pruning very often takes away the driving force from what is being discussed. This seems to me to be an important point, for frequently the clarification of a problem linguistically either presupposes the solution or by-passes the problem leaving it unsolved, and leaving in the minds of the reader a psychological hiatus. I am sure, on the other hand, that Mrs Arber would not advise loose writing as a lazy way of forcing one's problems on to other people.

The second part is concerned with the ultimate scientific process, that is, for biology, biological thinking. She discusses truth (from a Hegelian standpoint) and some of the basic assumptions of biology. She takes the law of uniformity of nature as a basic assumption, but does not always seem to be clear as to whether it is a sort of axiom in biological theory, a basic methodological postulate, or whether it is not axiom at all but a generalisation arrived at after adequate study. On page 95, she tries to get biologists to ask ' Why ' questions as well as ' How ' questions. This seems to me to be a great mistake, for it might possibly undermine the whole of one's views on what science is doing. Personally, I would not admit ' Why ' questions in science for their answer ends in metaphysics or theology. This was especially true of the example she gives. Further, since science progresses by the creation of descriptive explanations (in answer to ' How ' questions) we shall need new sorts of explanations or new sorts of science, and I am not persuaded that the present method would be improved upon by either of these alternatives. I found the discussion of unity, wholeness and Owen's notion of ' The one in the manifold ' difficult to associate with any clear concepts. Perhaps this is a case where there is too much brevity and not enough lucidity. We should be grateful if Mrs Arber could expand her ideas in this connection in the future.

The wealth of quotation and gentle thought make this book a delightful addition to any biologist's library.

R. F. J. Withers.

The Itinerant Ivory Tower, G. E. Hutchinson. (Yale University Press, New Haven ; Oxford University Press, London, 1953. Pp. xi+261. $4.00 ; 32s.)

Of this gathering of essays and critical notices, the Stirling Professor of Zoology at Yale says in his preface that ' inconsistencies of attitude may be detected when the work is considered as a whole. It is impossible to be completely consistent when face to face with the whole of nature in all her complexity for a period of years '. But neither the educated layman, who may perforce skip the most technical, nor the specialist will be a captious critic of these urbanely written articles.

There are in all twenty-three titles, ranging from a brief note on Ruth Benedict's *Chrysanthemum and the Sword* to an essay review of A. L. Kroeber's *Configurations of Culture Growth* and E. Huntington's *Mainsprings of Civilization*, and from a four page ' Note on the functions of a university ' —as a place of learning rather than of teaching—to a graceful memorial of D'Arcy Wentworth Thompson in a discussion of some points raised by *On Growth and Form*. And for good measure in this kind we are given some admonishment on rhabdomancy, a mildly derisive notice of the *Patterns of Sexual Behaviour* of C. S. Ford and F. A. Beach entitled ‘ The loves of the card index cards ’ and a gently hilarious exposure of some of the pseudo-scientific flying saucer literature published in the United States.

For readers of this *Journal* perhaps the most significant chapter is that on ‘ Methodology and value in the natural sciences in relation to certain religious concepts ’. Here Professor Hutchinson defines science, ‘ very broadly, essentially as the whole field of inductive knowledge in which the use of the scientific method is appropriate ’. The problem of the values involved in inductive knowledge he discusses in terms of the traditional dichotomy of ‘ pure ’ and ‘ applied ’ science, and once he gets outside scientific theory into the ‘ metatheory ’ of the subject, he intuitively sees ‘ that the statements of conceptual schemes of the inductive sciences are art forms and have . . . a property . . . commonly called “ beauty ” ’. This leads him to a consideration of the interaction of science and religion, on which problem he has an interesting and novel contribution to make.

E. Rowan Davies.

ERRATA AND CORRIGENDA

In ‘ Degree of Confirmation ’ by K. R. Popper, in the August Number, the following should be noted :

p. 144, line 6 *for* supports x *read* supports $\bar{x}$; line 7 *for* supports $\bar{x}$ *read* supports x.

p. 145, line 34 *for Forschsung read Forschung.*

p. 146, lines 10, 11, 12 *for* $>$ *read* $<$.

p. 149, line 5, *for* they *read* that they ; line 14, *for* 1. *read* 1, whenever $j < i$.

In ‘ The Age of the Universe ’ by B. Abramenko, in the November number, on p. 249, column 3, *for* R/R *read* $\dot{R}/R$; and on p. 252 in the field-equations *for* $2g_{\mu\nu}$ *read* $\lambda g_{\mu\nu}$. It should also be noted that on p. 249 ‘ constancy ’ in line 1 means uniformity of the model in any given epoch, but not its invariability in the course of expansion, for its value changes with time.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

Arthur F. Bentley, *Inquiry into Inquiries*, Edited and with an Introduction by Sidney Ratner, The Beacon Press, Boston, 1954, pp. xvi + 365, $6.

Ludwig von Bertalanffy, *Biophysik des Fliebgleichgewichts*, Verlag Friedr. Vieweg & Sohn, Braunschweig, 1953, pp. iv + 56.

Descartes : Philosophical Writings, Edited by Elizabeth Anscombe and Peter Thomas Geach, Thomas Nelson & Sons, Edinburgh, 1954, pp. lvi + 301, 12s. 6d.

Peter Fireman, *Perceptualistic Theory of Knowledge*, Philosophical Library, New York, 1954, pp. 50, $2.75.

Jacques Hadamard, *The Psychology of Invention in the Mathematical Field*, Dover Publications, New York, 1954, pp. xiii + 145, $1.25.

Gertrud Leisegang, *Descartes Dioptrik*, Westkulturverlag Anton Hain Meiseheim am Glan, 1954, pp. 168, brosch DM. 9, Ganzleinen DM. 11.

John Locke, *Essays on the Law of Nature*, Edited by W. von Leyden, Clarendon Press, Oxford, 1954, pp. xi + 292, 35s.

Ernst Mach, *The Principles of Physical Optics*, Translated by John S. Anderson and A. F. A. Young, Dover Publications, New York, 1953, pp. x + 324, $1.75.

Joseph Meurers, *Das Alter des Universums*, Westkulturverlag Anton Hain Meisenheim am Glan, 1954, pp. 103, Brosch DM. 6.70, Leinen DM. 9.

Armin Müller, *Die Grundkategorien des Lebendigen*, Westkulturverlag Anton Hain Meisenheim am Glan, 1954, pp. 207, Brosch DM. 14, Leinen DM. 16.50.

Arne Naess, *Interpretation and Preciseness ; a contribution to the Theory of communication*, I Kommisjon Hos Jacob Dybwad, Oslo, 1954, pp. xiv + 450.

Ruth Lydia Saw, *Leibniz*, Penguin Books Ltd., Harmondsworth, Middlesex, 1954, pp. 240, 2s. 6d.

J. W. Scott (ed.), *A Synoptic Index to the Proceedings of the Aristotelian Society 1900–1949*, Basil Blackwell, Oxford, 1954, pp. viii + 0205 + 127, £3.

S. G. Soal and F. Bateman, *Modern Experiments in Telepathy*, Faber & Faber Ltd., London, 1954, pp. xv + 425, 30s.

Harry K. Wells, *Pragmatism ; Philosophy of Imperialism*, Lawrence & Wishart, London, 1954, pp. 221, 15s.

(*b*) Journals

The contents of the latest issues of other journals are printed with the advertisements on the cover of this issue

Dialectica, Zürich, 1954, **8**, No. 30

P. C. Mahalanobis, ' The Foundations of Statistics '
W. Pauli, ' Wahrscheinlichkeit und Physik '

Methodos, Milano, 1954, **6**, No. 23
Hugo Dingler, ' Analyse oder Synthese in der Philosophie derWissenschaften ? '
A. M. Turing, ' Computing Machinery and Intelligence '
Samuel E. Gluck, ' The Epistemology of Mannheim's Sociology of Knowledge '
Silvio Ceccato, ' Ricordi di colloqui '

Philosophy of Science, Maryland, U.S.A., 1954, **21**, No. 4
Ernst Topitsch, ' Society, Technology, and Philosophical Reasoning '
E. A. Singer, Jr., ' Dialectic of the Schools II '
Richard L. Cartwright, ' Ontology and the Theory of Meaning '
James Bates, ' A Model for the Science of Decision '

Revue Internationale de Philosophie, Bruxelles, 1953, **8**, Nos. 1-2
P. Bernays, ' Zur Beurteilung der Situation in der beweistheoretischen Forschung'
A. Tarski, ' Resumé of the address of'
F. Gonseth, ' La preuve dans les sciences du réel '
R. B. Braithwaite, ' The nature of theoretical concepts and the role of models in an advanced science '
J. Clay, ' La vérification d'une théorie '
N. Bobbio, ' Considérations introductives sur le raisonnement des juristes ' (trad. par G. Goriély)
M.-T. Motte, ' La rigueur du raisonnement dans les débats juridiques '
Ch. Frankel, ' On the Nature of Proof in Philosophy '
G. Morpurgo-Tagliabue, ' La preuve au point de vue philosophique '
G. Ryle, ' Proofs in Philosophy '

Theoria, Madrid, 1954, **2**, Nos. 7-8
Editorial, ' The Spirit of Science '
Julio Rey Pastor, ' La teoría abstracts del lenguaje '
Alberto Einstein, ' Consideraciones sobre la teoria del conocimiento de Bertrand Russell '
Bertrand Russell, ' Réplica al trabajo de Alberto Einstein '
J. Robert Oppenheimer, ' La ciencia y el conocimiento común '
Francisco de A. Caballero, ' El caso del doctor Oppenheimer '
José Ferrater Mora, ' Wittgenstein símbolo de una época angustiada '
Miguel Cruz Hernández, ' La filosofía de Kant y la ciencia newtoniana '
Guiseppe Vaccarino, ' La fine della filosofia '
José María Sánchez Diana, ' Ideas españolas sobre la ciencia de la historia en el siglo XVIII '
José Gallego Díaz, ' Una neuva métrica como ensayo para axiomatizar la Economia '
Pedro Caba, ' El pensamiento y sus principios : la causalidad '
Julio Palacios, ' La definición de la Física '
Manuel Ubeda Purkiss, O.P., ' Ciencia antigua y ciencia moderna '
Miguel Sánchez-Mazas, ' La teoría del silogismo desarrollada en forma de álgebra '

(*c*) ARTICLES

Adam Abruzzi, ' Problems of Inference in the Socio-Physical Sciences ', *The Journal of Philosophy*, 1954, **51**, 537

RECENT PUBLICATIONS

F. Barone, 'L'empirismo logico di M. Schlick', *Rivista Critica di Storia della Filosofia*, 1954, **9**, 367

Rene Dugas, 'Relativité—Sur les pseudo-paradoxes de la relativité restreinte', *Comptes rendus des séances de l'Academie des Sciences*, 1954, **238**, 49

Arthur W. Burks, Don W. Warren, Jesse B. Wright, 'An Analysis of a Logical Machine Using Parenthesis-Free Notation', *Mathematical Tables and Other Aids to Computation*, 1954, **8**, 53

Adolf Grünbaum, 'Letter concerning Herbert Dingle's critique of the Bondi-Gold Cosmology', *Scientific American*, 1953, 6-8

Adolf Grünbaum, 'Science and Ideology', *The Scientific Monthly*, 1954, **79**, 13. Reprinted in *Humanist*, 1954, **14**, 161-173

Adolf Grünbaum, 'Operationism and Relativity', *The Scientific Monthly*, 1954, **79**, 228

Albert Hofstadter, 'The Myth of the Whole: A Consideration of Quine's View of Knowledge', *The Journal of Philosophy*, 1954, **41**, 397

Gail Kennedy, 'Science and the Transformation of Common Sense: The Basic Problem of Dewey's Philosophy', *The Journal of Philosophy*, 1954, **51**, 313

R. Matthey, 'Quelques réflexions sur le problème de l'évolution', *Scientia*, 1954, **89**, 200

Arne Naess, 'An Empirical Study of the Expressions "True", "Perfectly Certain" and "Extremely Probable"', *Hist-Filos. Klasse*, 1953, 6

Carmelo Ottaviano, 'Einstein filosofo', *Sophia*, 1954, **22**, 260-274

C. F. Presley, 'Laws and Theories in the Physical Sciences', *The Australasian Journal of Philosophy*, 1954, **32**, 79

Alfred Schutz, 'Concept and Theory Formation in the Social Sciences', *The Journal of Philosophy*, 1954, **51**, 257

F. V. Smith and Silloo K. Madan, 'A Projective Technique Based upon the Kinaesthetic and Tactile Modalities', *The British Journal of Psychology*, 1953, **44**, 156

F. V. Smith, W. Sluckin and D. Graham, 'The Efficiency of Differently Constituted Groups of Children in Different Types of Tasks', *The British Journal of Psychology*, 1953, **44**, 340

V. Somenzi, 'Scritti italiani di filosofia della scienza del quinquennio 1949-53', *Rivista Critica di Storia della Filosofia*, 1954, **9**, 500

W. Toman, 'Zum Problem der psychoanalytischen Theorie', *Studium Generale*, 1954, **7**, 473

V. Tonini, 'Il concetto di struttura nelle scienze moderne', *Scientia*, 1954, **89**, 37

E. Topitsch, 'Der Historismus', *Studium Generale*, 1954, **7**, 430

L. L. Whyte, D. L. Richards and D. Gabor, 'Velocity of Electron Pulses', *Nature*, 1954, **174**, 398

W. Yourgrau, 'General System Theory and the Vitalism-Mechanism Controversy', *Scientia*, 1952, **87**, 307-311